

Putting Drugs in Their Place

THE Misuse of Drugs Bill, now nearing the end of its journey through the British Parliament, should at least help to let people know what penalties they may incur by the use of illicit drugs. That at least is something to be grateful for. Over the years, but especially in the past decade, successive governments have responded to the emergence of new problems by passing new Acts of Parliament. It was in this spirit, for example, that the then government responded to the news in the Brain report of 1965 that over-prescribing might be an important cause of the spread of heroin addiction in Britain. The result was to set up drug treatment centres and, in the process, abandon the traditional British belief that a doctor could provide his own addict patients with supportive doses—registration became compulsory.

The Misuse of Drugs Bill will put this and other pieces of legislation within the same tidy framework. At the same time, it will introduce several innovations which are themselves responses to changing habits in the use of drugs in the past few years. First, the new bill defines three classes of drugs to which different penalties are in principle applicable. First come drugs such as heroin and opium as well as tetrahydrocannabinol, the essence of pot. LSD belongs to Class A. Cannabis, amphetamines and the like are in the second, less reprehensible category. Finally, in Class C, comes a set of drugs including benzphetamine which cause comparatively little havoc with life and which are comparatively lightly controlled. These categories resemble those suggested in 1968 by the WHO Expert Committee on Drug Dependence and the British government has evidently taken care to bring its practice conveniently into line with what may become an international convention.

Another innovation in the Misuse of Drugs Bill, more peculiar to Britain, is that a distinction is to be made between the offences of being found in possession of controlled drugs and being found guilty of supplying them to other people. Yet another development is that under the provisions of the new bill, it will be possible for police to stop people suspected of having drugs in their possession and to search them. Much of the concern which there has been in the past few months about the provisions of the bill centre around the way in which new developments like these are bound to affect not merely the control of dangerous drugs but also the sense in which the British concept of personal liberty is to be modified so as to make control practicable. Another issue is whether the new legislation will meet the criticisms of those who have cogently drawn attention to the inconsistencies of sentencing policy for drug offences in Britain.

On the classification of drugs, there is much to be said for what the government proposes. Until now, cannabis in all its forms has been lumped in with the opiates and cocaine under the Dangerous Drugs Acts of 1965 and 1967, and there have been notorious cases of people being sent to gaol for long periods for the possession of cannabis when others have been more lightly dealt with for having heroin in their pockets. So far, moreover, LSD and similar materials have been dealt with under the provisions

under the Drugs (Prevention of Misuse) Act of 1965 where possession without authority makes a person liable to six months' imprisonment by a magistrates' court, half the penalty for possessing the most serious drugs. Obviously it is proper that if the possession of heroin should be an offence against the law, presumably in the best interests of the offender or those who might emulate him, it is right and proper that LSD should be similarly dealt with.

Putting cannabis in a different category is also sensible, if only as a recognition that the cannabis habit is now so widespread in Britain that draconian penalties would be quite mistaken. Where the bill is on much less sure ground is in its classification of amphetamine (in Class B) and the barbiturates, which are not formally controlled but which are instead regulated by the conventions of medical practice. It is obviously proper that there should not be an illicit traffic in drugs which are also used in therapy. But is it possible indefinitely to ignore the extent to which dependence on barbiturates is now common and entirely legal within the framework of most general practices? After all, it is known that the amount of barbiturate prescribed by British general practitioners doubled between 1953 and 1959, and then amounted to more than 75 tons of the drug each year. It will no doubt be some time before cannabis is consumed on such a scale—if indeed the incidence of the regular use of cannabis in Britain is the 30 per 100,000 population estimated in Sir Aubrey Lewis's paper on the subject last year (Department of Health and Social Security, Reports on Public Health and Medical Subjects No. 124, HMSO, £0.375), consumption of cannabis in Britain is unlikely to exceed five tons a year. In short, sensible though the classification may be, its mere existence draws attention to the fact that illicit drugs, those defined in the new bill as controlled drugs, constitute only a part of the problem of drug addiction in Britain.

It is also clear that the new bill, when it becomes law, will be as open to misinterpretation by over-zealous authorities and magistrates as previous legislation on the subject. What precisely, for example, is meant by the distinction between somebody who has "a controlled drug in his possession" and somebody who is willing "to supply or offer to supply a controlled drug to another"? The intention, of course, is plain. Pushers are to be dealt with more fiercely than users. The difficulty is to be sure that policemen and magistrates will properly distinguish between supply for gain and supply for comradeship, one of the characteristics of pot smoking communities.

This is only a part of the problem of knowing how offenders against the regulations will be dealt with in the courts. The Wootton committee in 1968 was entirely right to complain about the scale of penalties applicable to cannabis offences, but the more serious objections is that these vary widely from one court to another. People do not know where they stand. Under the new regulations, in which conviction by a magistrates' court will entail a maximum of six months' imprisonment for a cannabis offence, it is likely that the same inconsistencies will be perpetuated, for there are certain to be some



courts which regard the maxima now established as norms and others which remember the Wootton committee's hope that it would be possible "to bring about a situation in which it is extremely unlikely that anyone will go to prison for an offence involving only possession for personal use or for supply on a very limited scale". The danger in all this, of course, is that a law which is inconsistent is also likely to be scorned.

The provisions in the bill which allow police to stop and search those suspected of a drug offence are equally open to abuse. Will some policemen regard long hair or a dishevelled appearance as a sufficient ground for suspicion? Will this part of the bill be an umbrella beneath which the police can harass those they do not care for? To be sure, given the ease with which small quantities of amphetamines or heroin can be disposed of, the power to search suspected persons is most probably necessary to the effective implementation of the law. It is more than probable that the British police will be as liberal in their use of the powers to be conferred on them by the new legislation as they have been in similar circumstances in the past. That said, however, it is only fair to recognize that the control of drugs now proposed does entail an important erosion of the principle that people can come and go as they please. That is something to be concerned about.

In this spirit, perhaps the most serious defect of the course which the British government is now following is that the clear and tough-minded legislation being enacted is not accompanied by the kind of social policy conducive to a long term solution of the problem of illicit drug use. No amount of legislation will turn heroin addicts into people who can do without the drug. If they are to escape from their dependence, they must be provided with the kinds of treatment that are known to be of some value. In circumstances like these, it would have been entirely appropriate if the new legislation had been accompanied by some sign on the government's part that it is prepared to devote substantial resources to the drug treatment centres which are, for better or worse, the only means by

which public authorities can now help addicts out of trouble. The difficulty, of course, is that methods of treatment are necessarily tentative and unsure, but it is ironical and even unjust that people should be sent to prison for having in their possession drugs without which life is to them insupportable. The fact that legislation may also help to drive away those who traffic in heroin and the hard drugs is of course important, but only a part of the story. And there is a very sober case for asking that the provision of medical treatment should be accompanied by more social research of a kind likely to throw light on the reasons why ordinary people become addicted to hard drugs.

The case of cannabis is different. In Britain, as in the United States, the incidence of the cannabis habit is comparatively high, especially among young people. There is a possibility that cannabis may lead to heroin, but nobody has convincingly distinguished between this pharmacological precept and the possibility that the cannabis smokers who finish up on heroin are those with a psychological tendency to drug dependence. It also seems likely that reports of teratological damage in laboratory animals dosed with cannabis have no relevance to actual use. To be sure, it is too soon to throw too much weight on this hypothesis, but there is at least a possibility that cannabis will turn out to be no more damaging than tobacco. So does it follow that cannabis should be put on free supply? That is what some people ask for. The case for saying no is chiefly that society is already plagued with problems of all kinds, and that to add another is at this stage pointless. It is only prudent to acknowledge, however, that from this point of view, it will be possible indefinitely to maintain that cannabis is a controlled drug only if the method by which it is controlled applies in much the same way to substances at present put on open sale, tobacco and alcohol among them. The predominance of cannabis users among the voting population of the 1990s will be a sufficient guarantee of that, and it is none too soon for the legal draughtsmen to be working on that problem.

New Look for NASA?

THE National Aeronautics and Space Administration, hardly the apple of the US Administration's eye in the past year, is likely to be further chastened by the report of the National Academy of Sciences committee on future strategy in space research (see page 142). With great solemnity, the academy's committee says what a great many scientists have been saying for years—that there should be a much more sober plan for the exploitation of the scientific instruments now at the disposal of NASA than any which the agency itself has put forward in the immediate past. In this spirit, of course, it is entirely sensible that the academy's committee should put the project to send a package of instruments on a distant journey around the solar system quite low on its shopping list. That project always had the appearance of a scheme devised in something of a hurry so as to provide a sense of daring in a programme lacking landings by people on the Moon. The truth is, of course, that flights by instruments taking several years to complete are unlikely to yield understanding of the nature of the solar system comparable with what will in any case be

provided by more conventional techniques. By this austere test, however, even the Viking programme that will send instruments to Mars in the next few years is comparatively unimportant—or extravagant in cost—and NASA should acknowledge as much. In brief, there are useful jobs to be done with the sum which NASA spends on what is classified as space science, but there remain doubts whether it is worth spending more than the Administration at present devotes to the National Science Foundation on a programme the urgency of which is determined much more by the availability of rocket motors of various kinds than by a rational allocation of priorities.

If NASA's scientific programme is full of inconsistencies, however, its handling of the much larger sums of money being spent on the space shuttle is even more bizarre. For all the money already spent on the development of rocket engines for this project, for all the plans included in the budget now being discussed in Washington and for all the words that have been written to justify the shuttle, it remains a puzzle to know what the shuttle

is meant to accomplish. To be sure, it is a fair prediction that in due course, some decades from now, people will have practical need to transport each other from the surface of the Earth to some orbiting apparatus and back again. In exactly the same spirit, it would have been possible to predict when the Wright brothers first flew that it would in due course be necessary to develop methods for constructing pressurized cabins capable of containing passengers safely at altitudes above 30,000 ft. The trouble then, which any small businessman would have pointed out, is that the time between successful development and practical application would have been uneconomically long. In business terms, such a strategy would have been a recipe for making a loss. To technologists, what it would imply is the squandering of valuable skill and resources. Nothing that NASA has said about the shuttle and its usefulness in the past year suggests to reasonable people that there is an application around the corner for such an expensive instrument. Rather, the agency seems to be following the old but now discredited precept that in technology, the wish can become the father of the thought, or at least that the availability of a new machine will somehow engender a demand for it. Outside the fields within which government

budgets operate, the road to Carey Street is strewn with opinions like these.

The appointment in the last few weeks of Dr James C. Fletcher as administrator of NASA should help to resolve some of the questions hanging over the space agency. The fact that Dr Fletcher has taken the job at all presumably implies that he has been given some assurance that the Administration is prepared to take the agency seriously at least for the next few years. Especially because the US Air Force is involved, the future of the shuttle as such is probably assured. But even within the framework now laid down, there is plenty of room for making better use of NASA's skill and experience. To begin with, all kinds of applications of space research are waiting to be carried out (which does not imply that even in this potentially valuable field, NASA should seek to run before it can walk). There is also plenty of room for some of the basic research on different types of rocket motors which may, ten years from now, provide the foundations for new rocket technology. More humdrum still, there is room for application of some of the advanced development in aeronautics from which new types of advanced aircraft are likely to emerge.

100 Years Ago



SCIENCE IN GOVERNMENT WORKSHOPS

THERE seems to be a singular antagonism between science and officialism. The Government has undertaken more than one special manufacture, and not without a certain measure of success, but even the best of Government factories are tainted with some perverse defiance of scientific principles. Why this should be is too large a question for present discussion, but the fact is beyond doubt. Take, for example, one of the most effective seats of national manufacture—the small-arm factory at Enfield, which is under the command of an officer who may fairly be credited with scientific intelligence. You will see there a considerable amount of what may be called imported science. Outside inventions in machinery, in rifle barrels, in locks, in breech-actions, and other branches of the work, have been appreciated, adopted, and improved, and this is so far good. The Snider is a very clever makeshift, and perhaps as good a converted rifle as could have been made out of the old Enfield. The projected Henry-Martini, again, has an excellent breech-action, though not quite the best that might have been selected. Its barrel is on a good and tried pattern, although one that has not very successfully competed with the Metford. In these respects, the design cannot be called unscientific, but it is said that one essential element of the new rifle—the sighting—if not absolutely left to be fixed by tradition and routine, will be in principle little better than the worthless sighting of the old Enfield. If a crack shot were offered the choice between a first-rate barrel with clumsy and unscientific sights and an inferior barrel fitted with perfect sights, he would certainly prefer to enter into a competition with the latter weapon.

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What Price Oil?

THE long haggle between a consortium of Western oil companies and a consortium of potential suppliers, principally Libya but including Saudi-Arabia and the Atlas states as well, raises important issues for fuel policy planners throughout the world. In the long run, the implications for science and technology may also be considerable. To begin with, what the Libyans ask for is what any supplier of raw materials would sensibly demand—a higher price for a commodity which even in the lush oil fields of North Africa is certain, one of these decades, to be exhausted. The trouble is that it will be possible to sell oil at a higher price only if this is done on a much smaller scale, for there are already signs that the price of oil is too high to make the commodity competitive. In Britain, for example, the days are gone when electricity generating stations might have been fuelled by imported oil, and there has even been a brief resurgence of the traditional atavistic fondness for coal which singled Britain out as such an odd place in the years immediately after the Second World War. In practice, in Britain, one result has been to stimulate the search for indigenous oil. Another has been to confirm the view that in the long run nuclear power is likely to be the anchor of the fuel economy. Much the same has happened in France, where only two weeks ago Electricité de France said that it would buy ten nuclear power stations in the next five years. It is inevitable that the haggle about oil prices will accelerate the tendency throughout the industrialized west to manufacture electricity from uranium. To say this does not of course imply that the bargain which the Libyans are seeking to drive will in the long run be to their disadvantage. But they will have to be more deft than they have been so far if they are to maximize their profits without pricing themselves out of the long term market.

OLD WORLD

Where Now With Nuclear Power?

THIS has been a troubled winter for the Central Electricity Generating Board's planners. Industrial disputes, the continuing breakdown of plant, increasing oil prices and soaring capital costs of nuclear power stations have all upset the economic balance between the four electricity generating fuels—coal, natural gas, oil and nuclear energy. But, in spite of the sharp increase in oil prices from the petroleum exporting countries, the CEGB seems still to be favouring oil-fired generating plant rather than nuclear plant. One of the chief factors in the energy equation is the fact that the capital cost of nuclear power stations is much greater than that of oil-fired stations: for about £200 million the CEGB can get an oil fired station on the Isle of Grain in the Thames Estuary with a capacity of 3,300 MW, while the proposed nuclear station at Sizewell B would cost £250 million to build, and would have a capacity of only 2,640 MW.

In such a situation, the relative generating costs of oil and nuclear power become increasingly important. Nuclear power in 1970 was, according to the CEGB (see Table 1), about half as expensive as oil as far as running costs are concerned, and the recent price increases announced by the oil producing countries may have tipped the overall balance in favour of nuclear power. To put the figures in perspective, oil and nuclear power will be competing for some 40 per cent of the UK generating capacity, now estimated to be about 54,000 MW maximum in the winter of 1975–76, coal fired generation will account for about half the capacity, and natural gas for about 5 per cent.

The CEGB's decision to defer construction of the Sizewell B power station and to go ahead with the Isle of Grain oil fired station (see *Nature*, 230, 7; 1971) indicates that, for the present at least, there is little change in the declared policy of maintaining a balance in favour of oil generation compared with nuclear power. This decision was taken in the light of revised forecasts for demand for electricity in the mid-1970s, which indicate that demand will rise by about 5 per cent a year compared with the original estimate of 6 per cent. But obviously bearing on the decision was the fact that estimated construction costs of the advanced gas cooled reactor at Heysham, Lancashire, increased by 37 per cent during the planning stage. In the

present economic climate, the CEGB therefore seems to be opting for lower capital costs.



Fawley Power Station

At present, there are three oil fired power stations under construction—the Isle of Grain, which will have a capacity of 3,300 MW, Fawley, which will have a capacity of 2,000 MW, and Pembroke, with a capacity of 2,000 MW. On the other hand, nuclear stations are being constructed at Dungeness B (1,200 MW), Hinkley Point (1,320 MW), Hartlepool (1,320 MW), and Heysham (1,320 MW). All these stations are AGRs, and in addition, the CEGB has received permission to go ahead with Sizewell B (2,640 MW). All this capacity should be installed by 1976, and, with the Sizewell B station in hand, the CEGB has some breathing space in which to assess the relative attractions of the various types of power station.

It has applications in the pipeline for four oil fired stations and three nuclear stations, totalling 7,200 MW and 4,250 MW respectively. The oil stations are scheduled for Inwork Point in Devon. Ince B in Cheshire, Brunswick Wharf in Tower Hamlets, and a giant oil station of 4,000 MW capacity at Killingholme on the Lincolnshire bank of the Humber. The scheduled nuclear stations are at Connah's Quay in Flintshire, a

2,500 MW AGR, Portskewett, near Cardiff, 1,300 MW AGR, and Oldbury B, near Bristol, which should be a high temperature reactor of about 650 MW capacity.

The decision to apply for permission for two more AGRs and one high temperature reactor underlines the CEGB's intention to concentrate on HTRs for its third generation of reactors, and to turn its back on commercial application of steam generating heavy water technology. This favouring of high temperature reactors—although an economic assessment of the various types of third generation reactors has not yet been completed—has already caused one of the two British nuclear consortia, British Nuclear Design and Construction Ltd, to drop its design work on the SGHWR. The Nuclear Power Group, on the other hand, believes that the SGHWR is a good export proposition, and its tender for the Australian Jervis Bay site stands every chance of being accepted.

The export record of the advanced gas cooled reactor has, however, not been a distinguished one. Apart from some interest shown by the Greek government, there has been a remarkable lack of foreign interest in British AGRs, and not a single export order has been received. This does not bode well for the export potential of the HTR, which is essentially a development of the AGR.

Part of the reason for the failure of the AGR on the export market has undoubtedly been the fact that its capital cost has greatly exceeded expectations. But this is in itself a result of the lack of orders that the nuclear power consortia have received—only the four CEGB stations under construction have been awarded to the consortia. Few savings have therefore come from repetition work, and a vicious circle has been set up in which the lack of export orders is not resulting in any savings, and this in turn makes the reactor even less marketable abroad. Unless this spiral can be broken from the beginning with the high temperature reactor, its prospects also look bleak.

Table 1 Power station operating costs, 1969–70

	Coal	Pence per kWh Oil	Nuclear
Fuel generation	0.5845	0.5985	0.1744
Fuel handling	0.0310	0.0330	0.0092
Operation	0.0527	0.0523	0.0560
Repairs and maintenance	0.0668	0.0688	0.0439
Total works cost	0.7350	0.7526	0.2835

Source: CEGB annual report 1969–70

ARCHITECTURE

Playing for Time

IN spite of all the alarms and diversions which have marked the progress of the school of the Architectural Association in recent years, the school seems determined to soldier on through yet another round in its battle for financial stability. Two months ago it seemed that all was lost when the Inner London Education Authority refused for the second time to recognize the school as a college of higher education and therefore to take financial responsibility for its existence. But now at the urging of the student body and with the full cooperation of the staff, a scheme of economies has been put forward designed to keep the school alive while a long term solution is sought.

These economies will be exercised chiefly in two directions: some of the school's full-time teaching staff will not be replaced when their contracts come to an end (and it is the school's policy to appoint staff for short periods only) but will be replaced by part-time teachers. It is also expected that quite considerable sums can be raised by sub-letting some of the school's accommodation. These measures, it is hoped, will cancel out and perhaps even reverse the overall deficit which in 1970-71 the President of the Students Union, Mr Patrick Hammill, said was about £50,000.

The future of the school has seemed shaky for many years now, but the quality and integrity of the teaching there have never been in doubt. Some 450 students, about 100 of them post-graduates, attend classes each year, representing about one-fifth of the total number of architects in training in Britain. Furthermore, the school achieves regularly the remarkable academic distinction of professional diploma pass rates of around 90 per cent. And, as Mr Hammill claimed, this is all done for about half the cost of a conventional university education. The school's fees are £460 a year which, on the basis of the size of the annual deficit, would seem to be about £150 too low. In at least one architectural department attached to a college of London University, the corresponding figure is about £1,200, made up partly from fees and partly from the Department of Education and Science.

Nevertheless the school, because of its peculiarly non-institutional status and robustly independent outlook, remains difficult to assimilate into conventional higher education. Protracted negotiations for a merger with Imperial College eventually fell through in 1970, chiefly it seems because Imperial College feared that the school's avowed intentions to stick to its democratic guns might prove a disturbing influence in the more re-

stricted organization of the University. Brief flirtations with other London colleges since then have proved no more successful.

So what for the future? Although for the time being the execution has been stayed, the future looks anything but bright. Two initial moves will be an approach to the DES for permission to raise fees—a move likely to be resented by local authorities who are already unhappy about the cost of educating an architect through the Architectural Association—and an attempt to relaunch the appeal first opened when a link with Imperial College seemed certain. There is about £200,000 promised or frozen in this fund and the school is busily contacting the donors for permission to use the money to fresh ends. The most important of these is, of course, accommodation. The lease on the Bedford Square premises ends in 1976, and there is as yet no indication that it will be renewed.

The outcome of this whole venture seems uncertain at present, but the school now has long practice in defying the financial and political pressures which seem to be leading irrevocably to its closure. Its supporters are sure it would thrive even if it were housed in a disused warehouse.

CERN ACCELERATOR

Scrutiny and Secrecy

SELDOM has a science policy decision taken by a British government been subject to as much public scrutiny as the decision to participate in the CERN 300 GeV project. In spite of this public exposure, however, did the Science Research Council and the government pay sufficient attention to the effects of the decision on other branches of science? According to Dr P. J. Smith (Open University), whatever the merits of the decision, it was simply the fruit of a "campaign of psychological warfare" conducted by nuclear physicists against the government, and insufficient regard was paid to priorities in science chiefly because there is no adequate framework for assessing such priorities. This thesis was received with some sympathy by many participants in the discussion about the 300 GeV accelerator, organized this week by the British Society for Social Responsibility in Science.

Few speakers at the meeting, Dr Smith included, declared themselves opposed to the CERN project, but many confessed that they are anxious about the procedure for taking such decisions. In particular, how can expenditure on such a project be set against expenditure on social services? To Professor E. H. S. Burhop (University College, London), the remark thrown out by Dr Smith that the government is

prepared to spend £3 million a year on the 300 GeV and yet to abandon the Consumer Council is incongruous.

The burden of Professor Burhop's argument was that the 300 GeV project is fully justified in the framework of nuclear physics—"there is no argument about the fact that high energy physics is worth pursuing and we cannot do it in a half-hearted manner"—and because there are still so many fundamental concepts in particle physics that are not clearly understood there will always be a case for spending more money on projects like CERN. But this esoteric argument was tempered with the suggestion that participation in projects such as CERN is a welcome development of international participation in science, a theme followed up by Dr R. Rosner (Rutherford), who defended the decision chiefly on the grounds that to abandon the 300 GeV accelerator would mean the end of European high energy physics.

Dr Rosner pointed out that the list of proposed experiments for the US 200 GeV accelerator at Batavia is already saturated, and the same is probably the case with the Soviet accelerator at Serpukhov; another large accelerator is therefore needed, and with the added advantage of superconducting magnet technology, the CERN 300 GeV project is worth supporting.

What the meeting should have been discussing, Dr Rosner suggested, was the scientific decision making process and not the merits and demerits of participation in one particular big science project. This suggestion was taken up with relish by other members of the BSSRS, although little in the way of fresh ideas emerged. The debate meandered slowly over the morality of defence research, took a look at the fact that some £2,000 million a year is spent on defence, and ended up on the doorstep of the Science Research Council.

Dr Smith, in his opening speech, suggested that the decision to participate in CERN owes its existence to the power of the nuclear physics lobby in both the Science Research Council and in the mass media. Such a situation essentially reduces science policy making to a situation of survival of the fittest, he said, and given such a situation, it is impossible to gauge the merits of a project in the light of other possibly equally worthy projects. And, even in the case of the 300 GeV decision, much of the debate in the Science Research Council was conducted behind closed doors, so that justice could not even be seen to be done. The impression left by the meeting was that although most of the participants were sufficiently convinced by the esoteric arguments for supporting the machine, many were worried about the obsessive secrecy that hallmarks much British science policy making.

INDIA

New Chief for INSA



DR B. R. SESHACHAR, professor of biology and director of the Centre for Advanced Research in Cell Biology and Endocrinology at the University of Delhi, has been appointed President of the Indian National Science Academy. Well known for his work on the cytology and cell biology of animals, particularly of protozoa, Dr Seshachar is chairman of the National Committee for Biological Science in India and of the Indian National Committee for the International Biological Programme. His chief field of study has been the organization of chromosomes in the macro-nucleus.

The Indian National Science Academy is the premier scientific society in India, discharging functions similar to those of the Royal Society, but also giving grants for basic research in India. It has 354 Fellows and about 38 honorary Fellows. Dr Seshachar takes over from Dr Atma Ram, Director General of the Council for Scientific and Industrial Research.

SOVIET UNIVERSITIES

Broad Expansion

from our Soviet Correspondent

THE training of scientists in the Soviet Union may be too narrow. That is one conclusion to be drawn from a recent article by the Rector of Odessa University, A. Bogatskii, published in *Pravda* (March 2, 1971) in connexion with the directives of the new five year plan.

The new plan calls for "necessary measures" to be taken to train "highly qualified specialists and workers". The majority of these, under the present educational system of the Soviet Union, will be trained in technical colleges and institutes of higher education. Professor

Bogatskii says, however, that the training offered by these institutions is too rapid and too limited. It is well known, he suggests, that there are considerable differences in the level of knowledge of the theoretical principles of their subject between those graduating from technical colleges and those from universities. In the chemical technology faculties of polytechnics, for example, students spend only about half the time on the fundamental chemical disciplines that they do in universities. Consequently, they can take in much less scientific information. Technical colleges, he claims, are more suitable for the training of specialists in "narrow, concrete, engineering subjects" for which the time allowed by the academic planners is quite sufficient. He urges, however, the further expansion of universities as the training ground of specialists of the future, for here the student will have a chance to acquire a profound knowledge not only of his own special subject but also of neighbouring disciplines.

University expansion, of course, has been going forward rapidly in the Soviet Union during the last five years. Professor Bogatskii's own University of Odessa, for example, now possesses nine faculties, 63 departments, several scientific research institutes, including an astronomical observatory, a botanical garden, palaeontological and zoological museums, a computer centre and many similar facilities. The academic and research staff now number more than 350, and there are 11,500 students. The high cost of expansion is, to a certain extent, offset by the earnings of the research institutes which undertake contract work for industry. (In 1970, says Bogatskii, 1.1 million rubles was earned from this source.) Many of the applied problems thus studied involve a number of departments and faculties, and are undertaken in conjunction with the research institutes of the All-Union and Ukrainian Academies of Sciences.

The close cooperation of academic institutions and industry is a basic tenet of Soviet educational planning. An industry-orientated approach to science has been particularly emphasized in the technical colleges and Institutes of Higher Education. While the importance of this close link is still paid some lip service, it is interesting to note that the current tenor of educational expansion seems to be towards universities in which students receive a more broadly based education.

AVIATION

Light Under a Bushel

THE new chairman of the relatively little known but very active Aeronautical Research Council is to be Professor Paul Owen, who holds the Zaharoff chair of aviation at Imperial

College, London. Professor Owen succeeds Professor A. D. Young, whose three-year term of office expires at the end of March.

The Aeronautical Research Council, set up in 1909 with the title of Advisory Committee for Aeronautics, has evolved into a body to which the Minister of Aviation Supply can turn for independent advice on all aspects of aeronautical research. Two of the reasons for its absence from the public gaze are that it has no executive responsibilities and does not disseminate funds.

Its independence is one of its chief assets, and although there are ten official members of the twenty-two strong council, the other twelve are appointed by the Minister from universities, the aircraft industry, government establishments and aircraft operators. The council itself, however, selects the 300 to 400 members of the twenty or so standing committees, special committees and subcommittees which have special responsibility for matters ranging from air traffic control to hypersonic flight.

The standing committee on materials and structure has, of course, proffered advice to the minister on carbon fibre research and development from time to time, and when the topic comes up for discussion by the council next month it should be in an excellent position to make far-reaching recommendations for a future which may well not see any contribution from Rolls-Royce.

OIL EXPLORATION

The UK Economic Bait

THE recent publicity over the search for oil now gathering momentum in Britain must have come as a surprise to anyone familiar with conventional geological ideas about the underlying structure of the country. There is certainly no evidence that giant fields like those of Alaska and the Middle East are hidden beneath the UK, but even if small pockets of oil were found trapped in faults, fractures and small anticlines, they could be very easily exploited. This is the economic bait that has attracted Texan and Canadian oil companies away from their more usual environments.

Although very successful abroad (or, perhaps, because of this success) British Petroleum, which has owned concessions on large areas of land in Britain for many years, has never carried out a full scale search for oil at home. An obvious parallel can be drawn with the situation in Holland, where a reasonably large oilfield lay undiscovered for decades while Shell pursued a successful policy of overseas exploration. As in Holland, parts of Britain close to the

North Sea have a geological structure which is just an extension of the region where large undersea gas fields have been discovered. The geologically promising structure underlying the English Channel may also extend into the south coastal regions of Britain, where the deepest test drilling so far planned has already commenced.

Rather more rarely, the geology of the rocks under the sea changes drastically at the coast. This is seen most clearly in the Cardigan Bay area, where a fault line coincides with the line of the coast, and gives rise to the possibility that even when seismic surveys at sea give unpromising results for oil prospectors, the nearby land might have an altogether different structure, concealing deposits of oil and gas. The chief problem which brings an element of chance into all this work is the difficulty of conducting detailed seismic surveys on land. At sea, it is simple enough to steer a ship on a straight course, throwing out explosive charges at regular intervals and listening for their echoes. But on land this form of surveying is severely restricted. So for inland oil searches, the usual technique is to find a reasonably promising area, and drill test wells without any really detailed seismic surveys, the results depending on luck to a fairly large extent.

Whatever becomes of the inland search for oil, the now well established oilfields in the North Sea should start producing oil for the home market in the next few weeks, as the weather improves. The BP/Gas Council strike seems able to produce 10,000 barrels of oil a day, so that there should be little difficulty in filling tankers at the well head (a tanker might hold some 1,000 barrels of oil), provided that the owners of the well are prepared to lose the natural gas associated with the oil. It would take rather longer to get the oil ashore by constructing a pipeline, but because so much oil is present, short term considerations will probably persuade the joint company to take advantage of the publicity value of bringing home the first tankers full of oil, perhaps before the end of April.

SRC

High Temperature Boost

THE latest in a series of reports published by the Science Research Council recommending support for a particular branch of science is published this week. It suggests that high temperature processes provide an exciting area of study which has a tangible economic payoff and is therefore eminently worthy of more special support from the coffers of the SRC (*High Temperature Processes*, available from the SRC). This latest exercise in making public the

rationale behind the SRC's decisions to single out particular areas for selective support follows hard on the heels of the report highlighting surface physics as a worthy subject to stimulate with an injection of funds, and the SRC is to be congratulated on breaking away from the tradition in other research councils of taking decisions behind firmly closed doors. But from the reports so far, it would seem that setting up a working party to explore the possibilities of a subject for selective support usually simply predetermines the fact that that subject will be found exciting and worthy.

The SRC has, however, had a fairly large finger in the high temperature process pie since 1966, when a previous working party suggested that the economic benefits of this research were such that it should get a special push from the SRC, and the present report is therefore chiefly a survey of the results of the 1966 funding. Little fault was found with the decision taken in that year to award large grants to Imperial College and the universities of Leeds and Oxford for high temperature studies and, indeed, the present report recommends that the grants to these establishments be renewed. In addition, the working party, which was under the chairmanship of Professor J. M. Coulson, University of Newcastle, has thrown out a few suggestions for other research projects that might now profitably be pursued.

Information about heat transfer from plasmas and flames is surprisingly sparse, the working party suggests, and therefore the SRC should consider offering a grant for work in this area. This programme of research should be supported and carried out in conjunction with the Rocket Propulsion Establishment—a similar link was suggested by the previous working party for the work on chemical flames being carried out at the University of Leeds. A major programme of research on using plasmas to produce fine particles is also deserving of support, and a speculative project concerned with high temperature organic synthesis should also be considered, the working party suggests. Finally, a programme to assess the commercial value of augmented flames and work in the fields of quenching and measurement should be looked at sympathetically by the SRC.

The chief rationale behind these recommendations is, of course, that high temperature process work is likely to have profitable industrial applications, and the working party believes that increasing industrial interest in this area bears out this suggestion. In particular, there is special interest in high temperature methods in the production of inorganic chemicals such as boron, metallic borides, phosphorus, silica, titanium dioxide and pure nickel.

Parliament in Britain

Lead in Petrol

THE average weekly intake of lead in Britain is 262 micrograms per person, only 52 of which are taken in through the air, and only 18 can in any way be attributed to lead in car exhausts. These figures were given by Lord Mowbray and Stourton in reply to a question from Lord O'Hagan. The figures were supplied by the Medical Research Council, whose Air Pollution Unit advises the government, but Lord Stonham was dubious about their accuracy, and he asked how they were derived. Lord Mowbray and Stourton said that they were obtained from measurements on one gentleman who was purported to be standing in Fleet Street for eight hours a day, another had to stand there for thirty hours in a week, another who just lived in London, and a fourth who lived in the country. The findings were averaged out to give a value for lead absorption in the country as a whole.

On the basis of these figures, Lord Mowbray and Stourton said that there is no need for urgent action to be taken about the content of lead in petrol. The government is, however, considering the problem. (Oral answers, Lords, March 10.)

Heart Disease

DR MILLER asked the Secretary of State for Scotland whether he intends to take steps to harden the water in areas where it is particularly soft in view of the fairly clearly established causal link between soft water and heart disease. But Mr Gordon Campbell replied that the connexion is as yet too imperfectly understood to serve as a basis for artificially hardening water supplies. Dr Miller suggested, however, that such action could prevent the premature death of some 500 people a year in the Glasgow area. (Written answers, March 10.)

Aerospace Launching Aid

IN reply to a question from Mr Merlyn Rees, Mr David Price, Under-Secretary of State, Ministry of Aviation Supply, gave details of government launching aid to aerospace firms during the past decade. The figures do not include expenditure on Concorde.

	£million
1960-61	6.05
1961-62	7.25
1962-63	9.86
1963-64	8.53
1964-65	6.07
1965-66	3.01
1966-67	2.30
1967-68	12.10
1968-69	18.96
1969-70	33.70

(Written answers, March 10.)

NEW WORLD

NASA Cocks Snooks at Academy Advice

by our Washington Correspondent

A LIST of future priorities for the unmanned part of NASA's space science programmes has been drawn up by a committee of the National Academy of Sciences. This is the first time an academy study has tried to set priorities across a broad range of NASA activities, in this case all the science-oriented projects directed by the Office of Space Science and Applications (OSSA). Although not asked by NASA for an opinion on manned space flight, the academy report states plainly that space science and applications by themselves are not enough to justify the cost of developing the shuttle, the vehicle on which NASA has pinned its hopes for keeping man in space after the Apollo and Skylab programmes have ended.

Convened by the Space Science Board of the National Academy, the report* sets out a list of projects to be undertaken in the next decade together with the priority NASA should assign to each within its available budget. These recommendations are likely to be urged on NASA with more than usual emphasis since the Space Science Board is for the moment the only formal channel through which the scientific community can influence the NASA programme. NASA's own in-house advisory system, including the broadly based Astronomy Missions Board and Lunar and Planetary Missions Board, were in effect disbanded in mid-1970, and a new system has not yet been instituted. The academy report was available to NASA planners in drawing up their budget for fiscal year 1972, in which the committee's advice has been followed in several minor ways and ignored in as many major aspects.

The report is the fruit of an unusual exercise in which some 90 scientists in seven major space science disciplines (planetary exploration, lunar science, astronomy, gravitational physics, solar-terrestrial physics, earth environmental sciences and life sciences) met for three weeks at Woods Hole, Massachusetts, last July and August and attempted to assign budget priorities to projects in their own disciplines under a fixed overall ceiling.

In as far as the procedure meant assessing the scientific merits of a large space telescope, say, against a mission to the outer planets, it seems to have resembled a parlour game with no holds barred. But skilful organization of the seven disciplinary panels and the execu-

tive committee which drew up the overall priority list appears to have defused several potentially grave squabbles.

Staff members of the academy interpret as a token of the committee's balance the fact that both astronomers and planetary scientists have claimed that the executive committee was stacked against them. And a chairman of a panel which was overruled on a crucial issue by the executive committee says that Herbert Friedman, chief scientist of the US Naval Research Laboratory's space research centre and chairman of the executive committee, "did a magnificent job, if not in making the thing work, at least in preventing it from exploding."

The job of the 15 man executive committee which was appointed by the Space Science Board was to meet with the seven panels both individually and in plenary session and encourage refinement or scaling down of their proposals. Further interaction was provided by having a member of the executive committee sit in on each panel session and report the flavour of the discussions. Finally, the draft list of priorities prepared by the executive committee was circulated in proof form before publication.

The priorities take the form of a list of new science projects that should be undertaken by NASA between now and the end of the decade. Excluded from consideration are the two Viking missions designed to land softly on Mars in 1975, planning for which is already at an advanced stage. The priorities are distributed into base, intermediate and higher budget missions, corresponding approximately to whether the budget for NASA's Office of Space Science and Applications (OSSA) attains to 100, 125 and 150 per cent of its 1971 budget excluding the Viking mission. Although at \$750 million the OSSA budget for 1972 is considerably greater than the \$566 million received in the present financial year, the Academy Space Science Board considers the 1972 budget to represent the base or lowest level since the increment is largely caused by increased funding for Viking.

Among the priorities set by the executive committee (for a full list see following article) probably the most surprising is the decision to dispense with the Grand Tour unless OSSA receives the highest of the three levels of funding envisaged. The decision, certainly one of the hardest the executive committee had to deal with, seems to have sprung in part from a fear

that under the present organization of NASA, large complex missions have a tendency to crowd out smaller missions, particularly during times of financial stringency, at the expense of a balanced programme. "NASA tends to favour large programmes rather than small ones," says a geophysicist member of the planetary exploration panel, "maybe because in the last stages of budget preparation there are more people fighting for large programmes."

A specific instance of a large programme growing at the expense of others is the Viking mission to Mars. According to Herbert Friedman, chairman of the executive committee, "Viking was endorsed by the academy when budget restraints were not nearly as tight as today. But if you asked the same group today they would probably come down against Viking." In short, having been bitten by Viking, scientists are twice shy of being taken by NASA for a Grand Tour. The escalating cost of the Viking programme is also said to have disenchanted NASA's in-house Lunar and Planetary Missions Board, and there is speculation that this change of heart did not enhance the standing of the now defunct board in the favour of NASA administrators.

How seriously is NASA taking the proposals made by the Space Science Board study? According to a NASA official, "We regard the report as a very fine set of scientific objectives that have been set out for us. I think you'll find we will have done all of them within the decade, though not necessarily in the sequence recommended".

Fine set of scientific objectives as the report may be, NASA cannot live by science alone, its devotion to which must be tempered by the needs to produce a politically acceptable programme and one that for preference keeps its existing staff and facilities reasonably well employed. Or, as the academy report notes, "From a management point of view, programme choices are influenced by the desire to fit missions to the special capabilities of NASA centres in an effort to preserve the balance of effort in various centres". These restraints have caused several deviations from the directions urged by the academy committee.

One of the chief recommendations of the academy report was that the Apollo missions originally designated 15 and 19 should be carried out as planned; these flights have been cancelled in order to save money for the Skylab and shuttle programmes.

* *Priorities for Space Research, 1971-80.* (National Academy of Sciences, Washington DC, 1971.) \$4.50

NASA officials also seem to have made a major misunderstanding of the three categories of priorities, corresponding to three possible budget levels, which is the backbone of the Space Science Board report. Testifying last week before the House Committee on Science and Astronautics, John E. Naugle, head of OSSA, justified the decision to proceed with the Grand Tour on the grounds that the academy committee had included the mission in their higher level budget programme and that OSSA's 1972 budget corresponds to this level.

Dr Friedman said last week that the new budget proposals made public after this committee had reported, correspond to the base level programme, from which the Grand Tour is excluded, since the increment in the OSSA budget for 1972 is due largely to increased funding for Viking. NASA's plans are in fact to proceed with development of the TOPS spacecraft with two craft being launched on the Jupiter-Uranus-Pluto route in 1978. There is the option either of sending another pair of missions via Jupiter to Saturn and Pluto in 1976 or of converting them into Jupiter probes.

Another of the report's recommendations that has so far been ignored is the high priority set on Planetary Explorer missions to Venus. There is a feeling among geophysicists that it is hard to sell Venus to NASA, perhaps because of a belief by agency officials that Venus has already been done by the Russians. In any event, apart from the Venus-Mercury fly-by in 1973, NASA will now miss the three launch windows for Venus in 1972, 1973 and 1975 and the earliest possibility for its next mission is 1977. The reason NASA has given Friedman for postponing a Venus launch is that the Goddard Space Flight Centre, the best laboratory at which to construct Planetary Explorer spacecraft, cannot undertake the work this year. Funding is also understood to be a problem.

Another major priority on which the academy committee has been overruled, at least for the time being, is in its recommendation that support for rocket and balloon based astronomy be doubled. Rocket based astronomy has in fact been reduced in the 1972 budget, and balloons receive the same as last year. NASA has made no response to the academy committee's suggestion that they cut some satellite in the programme in order to give greater support to rockets.

Although NASA has ignored the academy committee's advice on the three major issues, of the two scrapped Apollo missions, the Grand Tour and Planetary Explorers to Venus, as well as on the importance of rocket and balloon work, the agency has changed its plans on several lesser issues to accommodate the committee's wishes. In Dr Friedman's opinion, the major impact of the academy report on the 1972 budget proposals has

been in its advocacy of the high energy observatory (HEAO) which persuaded NASA and the Office of Management and Budget to let the programme go forward: "I suspect that without this support this would not have happened this year", Friedman says. OSSA's 1972 budget contains the first major funding for HEAO, launch of which is now scheduled for 1975, a year later than originally planned. Without the academy committee's support, the launch date would presumably have been postponed still further. A second HEAO flight is planned for 1976, with the possibility of two further flights before 1980.

Although the academy committee excluded from consideration the Viking missions already planned, it recommended that follow-on Vikings should be held in abeyance until the results of the first missions are analysed. NASA has accepted this advice, and also the proposal that work be started on development of the mirror for a large space telescope.

Perhaps the chief potential conflict between NASA's interests and those of the scientific community concerns the role of man in space. NASA did not ask the academy to consider the scientific usefulness of man in space, nor did the academy ask NASA that this most central question be included in its purview. Yet this is probably not an instance where the academy can be accused, as it has been in the past, of accepting a government contract on unnecessarily narrow terms of reference.

As a first step in setting priorities for NASA, the present task seems to have been as large as could reasonably be managed. In order to consider the shuttle programme as well, the economic justification of which will demand heavy use for military as well as scientific purposes, the 90 scientists participating in the study would have had to receive security clearance. In any event, even without a detailed study of the manned space programme or indeed NASA's invitation for its opinion, the academy committee has come out with a declaration that none of the new missions it proposes requires the participation of man. "We looked at what science we wanted done and when we had got our priorities we found all could be done without involving man," Dr Friedman explains. No recommendations are made about the shuttle or space station (Skylab), the academy committee says in its report, since the costs of these programmes are too vaguely defined to assess their value to scientific research; nevertheless, "it is clear that space science and applications by themselves are insufficient to justify the cost of developing the shuttle".

Elsewhere in its report, the academy committee expresses regret that the limitation of its study to OSSA programmes, "while probably necessary in

view of the magnitude of that task alone, did nevertheless prevent evaluation of the space programme as a whole. Thus, for example, the overall balance between manned and unmanned spaceflight was excluded from the terms of reference". It remains to be seen whether NASA will respond to this challenge by asking the National Academy to undertake such a study, or whether the agency will refer the issue to its revamped in-house advisory council when this is set up. In either case NASA may find that apart from those who wished to see the investment in Apollo fully exploited, the scientific advocates of man in space are mostly restricted to astronomers with an interest in having a man to operate their orbiting space telescopes.

Outside scientific advice is a commodity NASA will or should need for some time to come. Only now has the agency decided that its former internal advisory boards will be replaced by a Space Program Advisory Council, which will be divided into four committees dealing with applications, physical sciences, life sciences and space systems. The council has not yet been formed and even when it has been, in the opinion of a former member of the Lunar and Planetary Missions Board, "It takes a minimum of about two years for an advisory board to begin to become effective in translating what the scientific community is saying into concrete advice which is useful at the bureaucratic level in Washington".

How will the scientific community be able to influence NASA in the meantime? In the opinion of Michael B. McElroy, professor of atmospheric sciences at Harvard and chairman of the study panel on planetary exploration, "It is very important that some group continues to oversee what NASA is doing and points out any conflict. At present the only group that could do this is the Space Science Board. The Space Science Board is now an order of magnitude more important than it was last year."

But committees of the National Academy of Sciences, however forward they may be behind the scenes, cannot push their advice on a government agency unless formally asked. Will NASA dutifully consult the Space Science Board at every appropriate turn? "NASA desires this advice and will request it," Dr Friedman says. Certainly the scientific authority behind the academy committee's report will gain some attention for the priorities it has set forth. For instance, at last week's hearings before the House Science and Astronautics Committee, a weak but willing friend of NASA, the agency was asked to prepare written explanations as to why it proposed to ignore some of the academy committee's suggestions. At the very least, the Space Science Board has now put the shuttlecock in NASA's court.

Ranking of Space Missions

by our Washington Correspondent

FUTURE missions suggested by the seven disciplinary panels were ranked by the executive committee of the Space Science Board study (see preceding article) in three categories, base, intermediate and higher, corresponding to zero, 25 per cent and 50 per cent increments respectively in the size of the present budget of the Office of Space Science and Applications.

BASE LEVEL BUDGET

Planetary exploration Three Planetary Explorer missions to Venus should be launched, consisting of dual-entry probes in 1975, an orbiter for 1976-77, and another mission with either probes or an orbiter in 1978.

The base level budget does not allow a Grand Tour mission to Jupiter and the outer planets. Missions should concentrate on Jupiter, with spacecraft, possibly extended Pioneers, being launched to probe the planet's atmosphere, orbit it or "fly by, possibly towards one of the outer planets".

Lunar exploration Automated landers and rovers should be developed to continue where the Apollo programme leaves off.

Astronomy A series of high energy astronomical observatories (HEAO) with a payload of about 22,000 lb will take instruments above the atmosphere to explore cosmic rays and the gamma-ray and X-ray regions of the spectrum.

Small astronomical satellites (SAS) at a launch rate of one a year are intended to be simple, flexible satellites allowing exploitation of new developments in astronomy.

A doubling of support for rocket and balloon-based observations is "ranked with highest priorities in astronomy" since these are economical ways of studying the regions of the spectrum blocked out by the Earth's atmosphere.

An immediate start should be made on a large space telescope (LST) with a diffraction limited mirror of 1.5 m aperture.

Gravitational physics Priorities are for an earth-orbiting gyroscopic experiment now under development by Stanford University, and a sun-orbiting satellite designed by the European Space Research Organization (ESRO), for which NASA support will probably be requested.

Solar-terrestrial physics The interplanetary monitoring platform series (IMP) should be continued with a mother-daughter satellite system (IMP KK')

designed to study the interaction of the magnetosphere with the solar wind.

A solar-terrestrial probe should be launched into heliocentric orbit to monitor the undisturbed solar wind at a distance of some 10 million miles from Earth.

Priority should also be given to data analysis, funds for which should be doubled, and to solar-terrestrial physics, rockets and balloons, for which a 20 per cent increase in support is needed.

Earth observations Earth observatory satellites (EOS) will contribute to meteorology, Earth resources surveys, oceanography (sea state and surface temperature), hydrology and ecology.

Small applications technology satellites (SATS) are needed to permit rapid development of new approaches in meteorology and Earth resources work.

A dual satellite system whereby a low altitude, zero-drag satellite is tracked from a second in higher orbit would provide important data about gravity anomalies and mantle convection.

NASA should continue its support for Earth resources surveys from aircraft.

Life sciences On the assumption that manned flight continues, there should be stronger programmes of space medicine.

INTERMEDIATE LEVEL BUDGET

Astronomy Work on the 1.5 m aperture telescope should be accelerated, with the telescope ready for launch in the mid-1970's. Should there be a choice between the telescope and the Grand Tour, preference should be given to the telescope because of its higher scientific promise.

The L and M members of the orbiting solar observatory (OSO) series should be flown during the next high activity phase of the solar cycle.

Solar-terrestrial physics The F and G members of the atmospheric explorer (AE) series should be added to the already approved programme; AE, F and G are satellites with propulsion capability that

can maintain low orbits despite atmospheric drag.

Priority is also given to a magnetosphere experiment which would extend the IMP KK' concept by having four subsatellites in a tetrahedral configuration around the Earth.

Earth observations Development should proceed with an earth resources satellite (ERS), synchronous earth observation satellites (SEOS), and a drag-free Earth physics satellite capable of measuring sea surface heights to within 10 cm.

HIGHER LEVEL BUDGET

Planetary exploration A solar-electric mercury orbiter will follow up the exploration of the planet begun by the Venus-Mercury flyby already approved for 1973.

Priority is also given for the full Grand Tour concept, which calls for development of a thermoelectric outer planets spacecraft (TOPS) designed for longevity and operation far from Earth. Exploiting an unusual planetary alignment in the period 1975-80, TOPS spacecraft would use the gravitational field of Jupiter to swing on to the outer planets, two missions being dispatched to Saturn and Pluto (JSP) and two to Uranus and Neptune (JUN). Another TOPS craft would be swung back by Jupiter toward the Sun in a path taking it out of the ecliptic.

Lunar exploration A dual satellite orbital system should be developed to study the gravity anomalies and magnetic field of the Moon.

Astronomy The budget allows development of a solar observatory with one sec of arc pointing capability, and a kilometer-wave orbiting telescope (KWOT), consisting of a rhombic antenna surrounding an area some 10 km in diameter.

Other disciplines Also recommended in the higher level budget are four solar-terrestrial physics explorer satellites, each designed for a particular experiment, recoverable Earth resources satellites, and an improved biosatellite.

Table 1. Office of Space Science and Applications (OSSA) Budget Summary and Major Activities

	Funding (in millions of dollars)			Difference
	FY 1970	FY 1971	FY 1972	
Physics and Astronomy	113	116	110	- 6
Lunar and Planetary	151	145	312	+ 167
Space Applications	128	167	183	+ 16
Launch Vehicles/Bioscience	127	138	146	+ 8
Total	519	566	751	+ 185

NEWS AND VIEWS

Still More Light on Human Chromosomes

THE article by O'Riordan, Robinson, Buckton and Evans on page 167 of this issue of *Nature* is the latest and perhaps the most dramatic of the growing flood of publications reporting remarkable advances in human cytogenetics following the introduction of the technique of staining chromosomes with fluorescent agents. Only a few weeks ago the avenues opened up by the discovery of the intense fluorescence of part of the *Y* chromosome at metaphase, in interphase, and in the male sperm head were being discussed (see *Nature New Biology*, **229**, 225; 1971). This discovery (Sumner *et al.*, *Nature New Biology*, **229**, 231; 1971) has made possible the positive nuclear sexing of male cells (analogous to the sexing of female cells by the presence of a Barr body), a technique which is being actively exploited by workers in the field of antenatal sex determination. Since that date three more articles have appeared (*Nature*, **230**, 52 and 53; 1971); a brief note from Shettles suggesting the exciting possibility (not yet confirmed) that cells showing *Y* fluorescence may be found in mucus of the uterine cervix in the first three months of pregnancy (offering the chance of sexing without amniocentesis); and reports from Borgaonkar and Hollander and from Rook *et al.* which sound a note of caution—it is possible to have a *Y* which is functionally normal but which lacks a fluorescent segment, so that absence of *Y* fluorescent cells from the amniotic fluid cannot exclude the presence of a male embryo.

The whole technique of staining chromosomes with DNA-binding fluorochromes is a triumph for the veteran T. Caspersson and his colleagues at the Karolinska Institut, where the method has been studied and developed over a number of years. Caspersson has investigated several fluorescent acridine derivatives but has concentrated particularly on quinacrine mustard. He considered that quinacrine mustard might show preferential binding to DNA by virtue of a contained alkylating group, which was thought to react particularly with the N-7 atom of guanine, and thus might show up guanine-rich segments in chromosomes. Experiments on the chromosomes of plants (*Vicia* and *Trillium*) seemed to encourage this hope. Quinacrine mustard was concentrated in distinct fluorescent bands, and these correlated in position with chromosomal segments showing constrictions following cold treatment, and which were late-labelling with tritiated thymidine (both accepted tests for heterochromatin). This was encouraging because although a large increase in the proportion of guanine-cytosine base pairs in DNA will be needed to produce differential fluorescence, it is known that such an increase in the G-C/A-T ratio exists in the so-called satellite DNA which makes up 10–14 per cent of the total DNA of the mouse, and as much as 40 per cent of calf DNA. Satellite DNA is thought to consist of repetitive sequences in which chains of 200–300 nucleotides may be repeated 10,000–100,000 times.

Theory received a setback, however, when Caspersson found that quinacrine dihydrochloride, which lacks the alkylating group of QM and therefore should not bind specifically to guanine-rich areas, did in fact produce fluorescent bands at the same sites as quinacrine mustard, although the fluorescence was weaker. So did several

other fluorescent agents without alkylating groups (such as proflavine and acriflavine). These agents can intercalate into the grooves of the DNA double helix and can also bind ionically to DNA. So it is not at all clear at present what gives these agents their specificity.

Most of the workers on *Y* chromosome fluorescence have used quinacrine hydrochloride, which is similar to the dihydrochloride but less soluble, and which is better known as the potent antimalarial drug atebirin (mepacrine). Pearson, Bobrow and Vosa (*Nature*, **226**, 78; 1970) used this to demonstrate *Y* fluorescence in interphase nuclei because it was more readily available than quinacrine mustard. Now O'Riordan *et al.* have shown that it produces specific bands in human autosomes.

Caspersson's group was the first to demonstrate specific banding patterns in human chromosomes (it was in the course of this work that Laura Zech first reported *Y* fluorescence), and they were able to demonstrate specific banding patterns in most of the autosomes after staining with QM. These patterns were shown up by use of a reflectance photometer which could be moved along the length of each chromosome to give a wave pattern; and they considered that this was the method of choice, because the value of purely visual analysis was limited, partly by the difficulty of memorizing complex banding patterns, and partly by the difficulty of observing by eye the transition between the bands of fluorescence. O'Riordan *et al.* have now shown that this view was overcautious, and they make the remarkable claim that all twenty-two pairs of human autosomes can be identified visually and without recourse to fluorometry, after staining with quinacrine hydrochloride. This claim must be accepted, coming as it does from a unit which has probably looked at more human chromosomes than any other. The implications for human cytogenetics can only be described as colossal.

The recent situation regarding identification of the human autosomes has been that only pairs 1, 2, 3 and 16 could regularly and certainly be identified by observation and measurement. With the aid of autoradiography pairs 4, 5, 13–15, 17, 18, 21 and 22 could probably also be distinguished; but autoradiography is still very much a research technique, too time consuming and laborious for any routine use in autosomal identification. Now at one stroke it seems likely that every single autosomal pair can be identified, and by a technique which can be easily and quickly used by any laboratory with facilities for ultraviolet microscopy. The method will be of especial use in the identification of the hitherto intractable chromosome pairs 6–12 and 19–20.

O'Riordan *et al.* proceed to demonstrate the power of the technique by solving a problem of great theoretical interest—the identity of the "Philadelphia chromosome" found in chronic myeloid leukaemia. This has usually been considered to be the same as that which is triplicated in Down's syndrome (mongolism), both because of enzyme studies (as mentioned in the article) and because the incidence of this type of leukaemia is known to be higher in mongols than in normal persons. So the present finding is both dramatic and unexpected.

SELENOLOGY

Solar Wind Interaction

from our Geomagnetism Correspondent

ALTHOUGH there is no evidence to suggest that there is a significant lunar magnetic field at present, the Moon certainly seems to have possessed such a field at some time in the past. A magnetic intensity of 38 ± 3 gammas was recorded at the Apollo 12 landing site, for example; and rock samples brought back by both Apollos 11 and 12 have been found to contain magnetization. Both of these pieces of evidence show that some parts of the Moon are permanently magnetic—a phenomenon which requires the existence of a magnetic field at the time the particular rocks were formed. But there is also another likely consequence of permanent magnetism on the Moon, as Mihalov *et al.* point out (*Science*, 171, 892; 1971). The magnetic fields set up by the permanently magnetic rocks should, if large enough, deflect the charged particles of the solar wind which impinge on the Moon. The existence of any such deflexion will thus be further evidence for magnetic lunar rocks, assuming that other possible explanations can be ruled out.

Because there is no overall lunar magnetic field the Moon does not, of course, have a magnetosphere like the Earth; but, as the Explorer 35 results showed, there is a diamagnetic solar wind cavity behind the Moon which is bounded by a rarefaction wave. Mihalov *et al.* have found that adjacent to this wave there are sporadic but persistent maxima in the interplanetary magnetic field. If these maxima are produced by sources on the Moon itself (this is, of course, an assumption) then it is likely that the sources lie around the boundary of the Moon's hemisphere struck by the undeflected solar plasma. This is merely the result of the fact that the perturbations along the edges of the cavity formed by the Moon are most likely to be caused by effects at the edge of the lunar disk as "seen" by the solar wind moving outwards from the Sun. And given that the sources lie around the lunar boundary, it is possible to extrapolate the field maxima backwards to determine just where on the lunar surface the interactions with the solar wind take place.

Mihalov and his colleagues thus show that most of the lunar sources lie on the far side of the Moon and therefore by implication in highland regions rather than in the maria. The two largest, for example, each about 10^5 square kilometres in area, lie at about 0° – 15° S, 165° – 180° E and 15° – 30° S, 135° – 150° E, respectively. They do not therefore seem to be related directly to the mascons which tend to

lie in the centres of ringed maria on the near side of the Moon.

What then is the nature of these supposed lunar sources? Obviously they could be regions of permanent magnetism; but are there any other possible explanations? Mihalov and his co-workers actually consider several but rule them out as being less likely than permanent magnetism. One possibility is transient electromagnetic induction effects in the Moon's surface layer. It is known, for example, that overall electromagnetic induction in the Moon amplifies a part of the solar wind by a factor of up to 4; but the observed magnetic perturbations would have to be produced in regions where the lunar "crust"—as defined by low electrical conductivity—was thin. This is just the opposite of what would be expected from the locations of the sources. The highlands are thicker than the maria and thus on this basis are less likely to be the source of induction effects. Electromagnetic induction in conducting "islands" in the lunar crust can also be ruled out, according to Mihalov *et al.*, as can steady unipolar generators. A further possibility would be the effect of neutral gas

outlets producing charge exchange. But there is no evidence of tectonic or volcanic processes which might be accompanied by gas escape, least of all in the lunar highlands.

If the perturbations in the interplanetary magnetic field near the Moon's solar wind cavity really are the result of lunar interactions, the most likely explanation is thus permanent magnetism. This is so not only because other explanations are found wanting but because there is independent evidence for the existence of permanently magnetic rocks on the Moon.

INSECT CUTICLE

Hardening Mechanisms

from our Insect Physiology Correspondent

THIRTY years ago it was suggested by Pryor that the horny substance of the insect cuticle is protein sclerotized by a reaction formed between amino-groups in the cuticular proteins and quinones formed by the oxidation of catechol derivatives secreted into the cuticle. There is now much experimental evidence to support this hypothesis. According to Karlson and Sekeris and

Structure of Polyacrylamide Gels

ALTHOUGH polyacrylamide gels have been used extensively by biochemists over the past decade for the separation and characterization of macromolecules, there have been almost no attempts to determine their basic molecular architecture. A step in this direction has been made by Richards and Temple, a report of whose work appears in next Monday's *Nature Physical Science*.

Polyacrylamide gels are prepared by mixing a monomer (acrylamide) with the crosslinking agent (N,N'-methylene-bisacrylamide) in the presence of a polymerizing agent. The effect is to produce an insoluble three-dimensional network of monomer chains joined together by bridges. In water this network becomes hydrated—hence the term gel—as a result of the presence of the amide group. By choosing a suitable total concentration, T , or monomer plus cross-linking agent in which the proportions, C , of cross-linking agent to monomer is defined, it is possible to obtain a gel in which the holes or pores in the three-dimensional network are of similar size to, say, an average protein molecule. This gel can then be used as a molecular sieve to separate protein molecules of a given size range. Variations of the two parameters, T and C , also affect the physical properties of the gel, in particular its elasticity, turbidity and tendency to swell in excess solvents. Richards and Temple have measured the turbidities

and swelling factors of a series of gels of varying T and C and propose a model of gel structure to account for their observations.

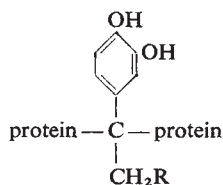
They define an ideal gel as one in which each bridge is connected to its four nearest neighbour monomer chains so that there are no free ends, loops or entanglements. Assuming a random spacial distribution of bridges for any given C , a value of T (called T_0) corresponding to an ideal gel can be calculated. If $T > T_0$, a crumpled gel is obtained in which there is too much monomer and the chains are less extended. Crumpled gels approach ideal gels if allowed to swell in excess solvent. If $T < T_0$, a clustered gel is obtained in which the monomer concentration is too low and the distribution of bridges is non-random and clustered.

For both clustered and crumpled gels Richards and Temple have calculated the effect of T and C on the swelling factor and turbidity and the theoretical curves agree qualitatively with their experimental data. In practical terms their results allow a choice of gel composition to optimize the desirable gel properties (such as ease of handling and low turbidity) and still produce a gel which approaches their ideal structure. To this end they recommend gels in which $C=5$ per cent. In clusters this is the value which has been most extensively used and is known to give the best sieving properties.

others, who have studied the hardening of the blowfly puparium, the immediate precursor of the tanning agent is acetyldopamine derived from tyrosine by way of dopa and dopamine; and it is generally believed that it is the quinone formed by the oxidation of acetyldopamine which tans the protein.

This is a violent and irreversible reaction in which the protein chains are believed to be bound directly to the benzene ring to give a hard and deeply pigmented product. But it has long been suspected that other forms of polymerization probably occur and give rise to sclerotin of a paler and softer kind. It is therefore very interesting that S. O. Andersen and F. M. Barrett (*J. Insect Physiol.*, **17**, 69; 1971) should have been able to obtain direct evidence for the existence of compounds of this sort. They have shown that several catechol derivatives can be isolated from acid hydrolysates of the hard cuticle of the locust *Schistocerca* and other insects. The two major components are a hydroxy-ketocatechol (2-hydroxy-3',4'-dihydroxy-acetophenone) and the corresponding aldehyde 3,4-dihydroxyphenylglyoxal.

The ketocatechols cannot be obtained from locust cuticle in the newly moulted insect. During hardening, when the proteins in both endocuticle and exocuticle are rendered insoluble, the amount of acid-released ketocatechols increases up to 3 per cent of the cuticle dry weight. The keto group, as judged from the ultraviolet spectrum, becomes released only during hydrolysis—from which it is inferred that the proteins are linked to the β -carbon atom of the aliphatic side chain of the catechol. Thus:



If R is $-\text{NHCOCH}_3$, this structure on hydrolysis will give arterenone, and if R is $-\text{OH}$ neutral ketocatechol will be obtained. Both products have been found.

This mechanism does not, of course, rule out the involvement of quinones also in cuticle sclerotization; these two processes can well take place simultaneously—quinone tanning perhaps for mass production; ketocatechol polymerization for more controlled and less violent sclerotization. Ketocatechols have so far been obtained from all insect species investigated but not from any other arthropods. Still further mechanisms for hardening may well exist.

MUSCLE

Where's the Myosin?

from our Cell Biology Correspondent

THE enormous progress which has been made during the past two decades in our understanding of the structure and functioning of striated muscle is an object lesson to all cell biologists seriously determined to understand the molecular architecture of cells. So much is known about how muscle is organized and functions because electron microscopic observations have been combined and correlated with X-ray diffraction studies; data obtained by one technique have not only lent credence to that obtained by the other but also each approach has complemented the other, filling in the gaps inevitable when only one method is used alone. Of course, the structure of striated muscle is ideally suited to analysis by these two techniques and when it comes to analysing in the same way other biological structures of greater biochemical complexity and less regular and rigid architecture

we can expect the going to be far tougher. For example, the molecular architecture of even smooth muscle, the biochemistry of which, not surprisingly, is basically similar to that of striated muscle, is proving decidedly more difficult to sort out.

Like striated muscle, vertebrate smooth muscle contains the three proteins actin, myosin and tropomyosin but the crucial—and unsolved—question as far as models of contraction are concerned is how these proteins are aggregated. Electron microscopy of sections of smooth muscle fixed and embedded by the procedures now conventional for striated muscle reveals thin actin filaments but does not reveal corresponding thick filaments characteristic of myosin aggregates. It is becoming increasingly clear, however, that thick, presumably myosin, filaments can be seen in smooth muscle which has been subjected to various manipulations. Last year, for example, Rice *et al.* (*J. Cell Biol.*, **47**, 183; 1970) reported observing thin actin and thick myosin filaments in contracted *taenia coli* muscle fixed in hypertonic

A Liquid Phase in Polycrystalline Ice

THE first evidence for the existence of a liquid phase in polycrystalline ice came to light about three years ago (for example, *Chem. Comm.*, **17**, 323 and 880; 1967), and in next Monday's *Nature Physical Science* the topic is further examined in some depth and from different points of view.

Several explanations of the rather unexpected liquid phase have been put forward, including the postulate of a thin surface liquid film and of a surface energy at grain corners which causes local freezing point depression; impurities and interstitial water molecules have also been thought responsible by several workers. Bell *et al.* describe their proton magnetic resonance (PMR) measurements on ice prepared under a number of conditions but feel unable to commit themselves to any of the available theories.

Paren and Walker, however, favour the explanation based on the limited solubility of inorganic compounds in ice; this phenomenon arises because few inorganic compounds are capable of incorporating themselves to any great extent into the ice structure. They are either unable to form the necessary hydrogen bonds with the water molecules or they are the wrong size or both. As a consequence, liquid inevitably forms within ice if the concentration is above the very small solubility limit (a mol fraction of about 10^{-7} or less for the salts in seawater, for example). Even very carefully prepared water samples, deposited from the vapour

phase into a clean quartz sample tube and then frozen, gave a PMR signal corresponding to about 0.1 per cent of liquid within the ice.

It has been suggested that the liquid is in the form of intergranular fillets and Paren and Walker support this hypothesis although they admit that intra-granular inclusions are also a possibility. They point out that at very high impurity concentrations, the total electrical conduction near the melting point is dominated by the brine veins, but that at impurity concentrations nearer the solubility limit more and more of the conduction can be ascribed to the solid solution. Paren and Walker have been able to deduce from electrical evidence that the solubility limit for sea salts is about 4×10^{-8} mol fraction.

As far as mechanical properties are concerned, a fairly simple relationship between creep rate and applied stress seems to hold for polycrystalline ice between -50°C and -10°C , but at higher temperatures the rate is greater than would be expected. Single crystals of ice, however, behave predictably up to 0°C , and Paren and Walker suggest that the liquid phase softens ice polycrystals when the solubility limit is exceeded. The implications for glaciology are obviously important, and although veins have been suggested previously as a means of draining off soluble and gaseous impurities from temperate glaciers it now seems possible that a similar mechanism is also at work in colder glaciers.

glutaraldehyde (6.25 per cent). And Lowy and Small (*Nature*, **227**, 46; 1970) observed thick ribbon-shaped filaments of myosin in guinea-pig taenia coli muscle. But the muscle had been immersed for 3–4 h in Ringer's solution at 0° C, which eliminated the contractile response, before it was fixed conventionally. Now Rosenbluth (*J. Cell. Biol.*, **48**, 174; 1971) reports that after exposure to a solution of trypsin in Ringer's, small intestinal smooth muscle of the toad *Bufo marinus* contains thick filaments about 250–300 Å in diameter and some 0.5 to 1.0 µ long. Further, some of these filaments have an axial periodicity of 143 Å and they closely resemble aggregates of smooth muscle myosin produced *in vitro*. In the cells treated with enzymes, however, these thick, presumably myosin filaments are segregated from the thin actin filaments.

Rosenbluth's interpretation of all this, which may not be welcome in all quarters, is that in smooth muscle *in vivo* the myosin occurs dispersed among the actin filaments in a relatively disaggregated state. And by implication the thick myosin filaments he and others have seen are artefacts of the preparation techniques, which induce aggregation of myosin. It will be interesting to see if such nihilism survives the test of time.

In the same issue of the *Journal of Molecular Biology* (*ibid.*, 128), Shimada published a series of electron micrographs which illustrate the process of myoblast fusion, the process which apparently gives rise to multinucleated muscle fibres during embryogenesis. Shimada's observations of cultured myoblasts reveal that myoblasts destined to fuse become closely applied. The two cells pinocytose projections from each other's sarcolemmas; tubules and vesicles can then span the two cells and eventually their surface membranes completely disappear at the site of contact so their cytoplasms become confluent. Such fusions occur between pairs of uninucleate myoblasts multi and uninucleate cells and nascent multinucleate myotubes. Clearly, they pose some fascinating questions about the mechanisms of cellular recognition, adhesion and contact, all of which are presumably mediated through the chemistry of the cell surface.

MAMMALS

Bats in the Attic

WITH the exception of rodents (order Rodentia) more species of bats (order Chiroptera) have been recorded than any other order of mammals. And among terrestrial mammals, bats of the genus *Myotis* in the family Vespertilionidae are surpassed in their distribution only by man. This genus is

absent only from the Arctic, subarctic regions and some oceanic islands. But in spite of being relatively accessible to zoologists who care to look for them, surprisingly little is known about the life habits and ecology of bats, and this applies particularly to species living in neotropical regions. Zoologists will therefore be glad of Don E. Wilson's study of the ecology of *Myotis nigricans*, a small vespertilionid species which is limited to the New World tropics and is locally abundant in the Panama Canal Zone (*J. Zool.*, **163**, 1).

Wilson was fortunate in being able to study an easily accessible roost of more than 1,000 individuals in the attic of a laboratory building on Barro Colorado Island. Roosting in buildings is not unusual for this species. In fact, it seems to prefer man-made structures and other disturbed habitats to primary forest, which suggests that it is a relative newcomer to the American tropics. Caves and hollow trees, the two most common roosting sites for bats, are heavily used by many species of phyllostomatid and emballonurid bats and it is possible that if *M. nigricans* entered the tropics after these other species were well established in primary forest it may have been forced to roost in unused areas such as attics and under the eaves of thatched roofs. The theory that this species arrived recently in the tropics is supported further by the bat's food preferences. It is insectivorous and it is possible that it finds

a larger food supply around cleared areas and human habitations than in primary forest where there are fewer insects between the ground level and up to about 10 m compared with inside the canopy. Of course, *M. nigricans* may simply have been a rare species before the clearing of forests and the spread of agriculture and human settlements (perhaps because of competition for roost sites) and has only recently increased in numbers.

The daily thermoregulatory activity of *M. nigricans* is interesting. As the tin roof of the laboratory building warms up each afternoon (except in the cooler part of the rainy season from September to December), Wilson noted that the bats break up their roosting clusters under the roof and move down the walls to the floor where there is better air circulation. They then congregate in different corners of the room depending on the time of year and the wind patterns. When under the roof, most of the bats are in one of several clusters, but there are also some solitary roosters. Wilson found that females predominate in the clusters and it is the males which tend to be solitary. When the clusters break up in the hot weather from January to September the sexes therefore mingle on the floor and this is when copulation occurs. No copulation takes place in the latter part of the rainy season

Salt Layer Beneath the Mediterranean

IN next Monday's *Nature Physical Science* Auzende and his colleagues at the Centre Océanologique de Bretagne, Brest, describe a continuation of their work on the interpretation of diapirs as salt domes, this time in the Mediterranean. Many people have reported the presence of diapirs in this area but have never been able to prove conclusively that they are the result of salt doming. To be sure, the circumstantial evidence for this is strong—notably the presence of sulphur, chalcopyrite and increased salinity in the interstitial waters. But the structures are small (usually less than 2 km wide) and covered by about 3 km of sediment which makes them not easily accessible. Auzende and his co-workers have thus extended their indirect observations using the 'Flexotir' deep seismic profiling technique.

They have been able to infer that a salt layer beneath the sediment underlies a great part of the western Mediterranean at least. The bottom of these evaporite deposits appears as a strong reflector, flat or slightly folded, in contrast to the upper boundary which is, as expected, often strongly

disturbed by diapirism. Away from the domes, however, the salt layer seems to be more or less uniform in thickness and at a constant level.

The existence of such a salt layer inevitably raises the question of its age. Extension of the continental geology suggests two possibilities here—Triassic or Oligo-Miocene—and both have been offered support at some time or other in connexion with a hypothesized salt layer. Glangeaud *et al.* (*Bull. Soc. Geol. France*, **8**, 921; 1966), for example, suggested Triassic whereas Ryan (thesis, Columbia Univ., 1969) plumped for Oligocene. Auzende and his colleagues conclude from sediment thickness and tectonic evidence, however, that the layer is, in fact, upper Miocene in age.

All this work was completed and the conclusions drawn before the JOIDES vessel, Glomar Challenger, entered the Mediterranean. But in a footnote Auzende *et al.* point out that in the meantime salt has been recovered from three holes in the western Mediterranean during Leg XIII of the JOIDES project; and that the upper Miocene age has been confirmed.

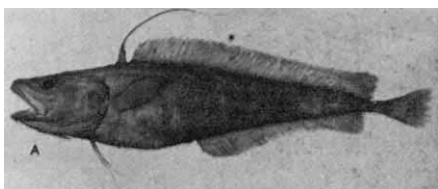
so that no young are weaned in the dry season (January to March) when food is short. Although *M. nigricans* is poly-oestrus and it breeds continually from January to September, the males unusually show a resting phase in the spermatogenic cycle from October to December.

Gestation in *M. nigricans* lasts for 50 to 60 days and like most species of *Myotis* there is only one young per litter. The young are naked and remain so for a week. They seem to remain attached to their mother for the first 2 to 3 days after birth, but on the third or fourth day they remain in the roost while the mother feeds. During this time they remain clustered together in large groups and the mother is able to pick out her own baby when she returns to the roost from feeding about an hour before sunrise. Weaning occurs at 5 to 6 weeks, by which time the bats are able to fly, and many of the young, particularly the males, leave the roost at this time to fly to other roosting sites. Wilson found that numerous small roosts on the island are composed almost entirely of subadult males, which suggests that males are territorial to some extent and form roosts to which females are attracted.

DEEP SEA FISH

North Atlantic Morids

from our Marine Vertebrate Correspondent



Lepidion guentheri from Madeira, standard length 413 mm (from *J. Fish. Res. Bd. Canad.*, 27; 1970).

THE morid fishes are often known as the deep sea cods, an apt term for they are related to the cod fishes which are so abundant on the continental shelf, but supplant them in the deep sea, especially close to the sea bed. It is principally because of their bathymetric distribution and their relative sparsity in biological collections that the taxonomy of deep sea cods has become confused, and little is known of their biology. For this reason the careful analysis of one of the more common North Atlantic genera, *Lepidion*, by Wilfred Templeman (*J. Fish. Res. Bd. Canad.*, 27, 457; 1970) is most welcome for it has cleared up a considerable body of confusion among the species occurring in this area.

Templeman's interest in the genus was stimulated by the capture of

several specimens between 1953 and 1969 from Labrador to the south-eastern Grand Bank area. These were the first *Lepidion* to be taken in the western Atlantic. The problem then became one of finding the correct name from a plethora of synonyms applied to eastern Atlantic *Lepidion*, and of deciding if the new specimens were the same species as those already described. Following a careful analysis of the new material and of the collections in a number of institutions, Templeman has concluded that his western Atlantic specimens are identical with *Lepidion eques*, a species known otherwise from the Bay of Biscay northwards along the Atlantic continental shelf to Iceland and east Greenland. It has only been found in numbers in depths of between 500 and 1,300 m.

Ice Nuclei at Hawaii

OBSERVATIONS at Hawaii of so-called storms of ice nuclei are reported by Hobbs, Fullerton and Bluhm in next Monday's *Nature Physical Science*. A knowledge of the concentration, nature and mode of action of ice nuclei is of importance to the cloud physicist because much of the world's precipitation is formed by the so-called Bergeron-Sindeisn process in which ice particles play a central part. Because small droplets of water do not freeze spontaneously until a temperature as low as -35 to -40°C is reached, many clouds in the atmosphere are composed largely of supercooled droplets with temperatures below 0°C . In the Bergeron-Sindeisn precipitation forming mechanism a small proportion of these supercooled droplets freeze and, because the equilibrium vapour pressure over ice is significantly lower than that over water, the ice particles so formed grow by sublimation at the expense of the surrounding water droplets. Once they have grown to such a degree that their fore speeds are greater than those of the water droplets their growth is accelerated by collision and coalescence with the droplets. The ice crystals can also collide with each other to form snowflakes. As they descend below the 0°C atmospheric level, the ice particles melt and become rain drops.

For this mechanism to be operative, a small fraction of the supercooled cloud droplets must freeze at much higher temperatures than those expected from spontaneous freezing. This is brought about by small particles which when incorporated in, or brought into contact with, the supercooled droplets cause them to freeze. These particles are called ice nuclei or freezing nuclei. Attempts to increase

A second, and closely related, species, *L. lepidion*, is found in the northern part of the west Mediterranean basin in depths ranging from 600 to 2,230 m. A third species, *L. guentheri*, is known only from the west coast of the Iberian peninsular, Madeira and the Azores, and although this name has been widely used for specimens of the genus in the Mediterranean and northern Europe, Templeman regards them all as having been misidentified. The fourth species of north Atlantic *Lepidion* is, however, still enigmatic. Captured on deep water long lines in the Bay of Biscay in 1966 and 1967, they were described by G. R. Forster of the Plymouth Laboratory (*J. Mar. Biol. Ass. UK*, 48, 479; 1968) as *L. guentheri*. The problem with these two fish was simply the size, for both were more than a metre in body

rainfall by the introduction of artificial ice nuclei, such as silver iodide, into suitable clouds have been made chiefly on the basis that there is a shortage of ice nuclei in the atmosphere. In the Soviet Union, extensive "cloud seeding" is also undertaken to increase the number of hailstones falling from a cumulonimbus cloud and so reduce the size of the stones and the damage done to crops.

With this background in mind it will be appreciated that a vast literature has been built up as the result of work in many countries on the nature, sources, concentrations and mode of operation of ice nuclei. The observations by Hobbs and his co-workers on the variability and concentration of ice nuclei at ground level at Hawaii over a limited period of five weeks during the summer of 1969 do not make a particularly exciting contribution to that literature but they do typify the difficulties of interpreting observations of this nature. The reported concentrations of about $700\text{ ice nuclei m}^{-3}$ at -21°C during the peaks of the so-called storms is well within the range of median concentrations of nuclei found at some forty-four places around the globe in a survey by Biggs and Stevenson (*J. Rech. Atmos.*, 4, 41; 1971) during the first three months of 1969. The mean value at all stations in this survey was 600 m^{-3} or $1,900\text{ m}^{-3}$ at -20°C depending on the counting method used. The diurnal variations at Hawaii are no greater than the short term fluctuations found elsewhere. For instance, in Europe and Japan, air containing high concentrations of ice nuclei often seems to have passed over areas of heavy industrial activity so the concentrations are clearly very sensitive to wind direction and atmospheric stability.

length whereas the other species rarely grow to even 50 cm. Templeman tentatively assigns these giants to *Lepidion schmidtii*, a species originally described from Sagami Bay, Japan, at a depth of 550–700 m and hitherto known only from this locality. Distributions of this kind are not uncommon in deep water species of fish. For example, the related morid species *Antimora rostrata* occurs in deep water in the North and South Atlantic and in the northern and southern parts of the Pacific Ocean.

As well as clarifying the nomenclature of these deep sea fish, Templeman has taken the opportunity to make some biological observations. *L. eques* begins to spawn in April in the Icelandic region, and reaches sexual maturity at a shorter length in the higher temperature areas of its range. Supplementary data from this study (*Tech. Rep. Fish. Res. Bd. Canad.*, No. 160, 1; 1970) demonstrates that this fish eats chiefly crustaceans, with polychaete worms as a minor item of diet. The total absence of fish in their stomachs is interesting but is probably a reflexion on the build of *Lepidion*—heavy bodied anteriorly and slim tailed, which does not suggest a fast swimming, agile predator.

CONSERVATION

Knysna Elephants

from a South African Correspondent

THE most southerly surviving herds of African elephants, *Loxodonta africana*, are those of Addo and Knysna, along the southern coast of the Cape Province. To the north the nearest elephants are found in northern Zululand, nearly 600 miles away as the crow flies.

The Addo herd, under the excellent protection of the National Parks Board, is probably comparatively safe and at present numbers some fifty-four animals. The Knysna herd, on the other hand, is teetering on the edge of extinction. As far back as 1908, it numbered less than twenty individuals (Kinloch, *Afric. Wild Life*, 22, 185; 1968), and in 1920 the herd was further depleted by a professional hunter, Major P. J. Pretorius, who destroyed five animals, bringing the herd down to thirteen individuals. Most estimates today place the number at around ten or eleven animals. Concern for this herd has been strengthened by the belief, expressed by Major Pretorius among others, that it represents a different race from the Addo one.

For some years now the Eastern Province Branch of the Wild Life Protection and Conservation Society of South Africa has been concerned with the fate of the Knysna herd. In 1968 it held a symposium on the future

of the Knysna elephants, and as a result employed a professional game ranger, Mr B. Carter, to undertake a survey of these animals. His report (*Knysna Elephant Survey*. Wild Life Protection and Conservation Society of South Africa, Eastern Province Branch, 1970) stated that there were "at least ten elephants in the forest and probably no more"; that they did not appear to represent a separate subspecies, and that, to protect elephants and humans from each other, the elephants should be fenced off in the Harkerville Forest.

At this stage the campaign met with unexpected opposition, from conservation-oriented botanists, who were upset at Carter's prediction that the elephants would eventually open up the forest into glades. Their reaction was that, given a choice between the elephants and the forest, they regarded the forest as more important, and if saving the elephants meant damaging the forests, they were not at all keen on saving the elephants. A meeting was hastily convened and held in Grahamstown on October 30, 1970, well attended by biologists and conservationists, particularly from the eastern Cape Province.

A certain amount of discussion took

place as to how many elephants there were, their population dynamics, how much they ate, whether they were being poached (a point which was not resolved), whether it would be possible and practical to confine them to Harkerville, and so forth. One of the delegates roundly stated that in his opinion the Knysna elephant did not have as high a conservation priority as the Harkerville forest itself. The eventual proposals of the meeting made little mention of elephants and instead expressed concern primarily over the future status of the entire ecosystem of the southern Cape indigenous forests (which would presumably include the elephants) and the support the Wildlife Society should give to the conservation of these.

The Wildlife Society representatives at the meeting, although agreeing in principle with its recommendations, were not prepared to speak on behalf of the society. This is perhaps not surprising, because the views expressed seem to suggest that they have not placed the emphasis correctly in their conservation attempts—in effect, that they could not see the forest for the trees (or the elephants).

Multiple Mutations in Nitrosoguanidine Roulette

IN next Wednesday's *Nature New Biology*, N. Guerola, J. L. Ingraham and E. Cerdá-Olmedo shed new light on the susceptibility of the replicating region of DNA to mutation. This work follows from an earlier publication by E. Cerdá-Olmedo, P. C. Hanawalt, and N. Guerola (*J. Mol. Biol.*, 33, 705; 1968) which showed that treatment of *Escherichia coli* with nitrosoguanidine increases the mutation frequencies of genes which are in the replication region at the time of treatment. In fact, the increased frequency of nitrosoguanidine induced mutants can be used to detect the position of the replicating region. If cultures are synchronized and treated at successive times the sequence in which the genes mutate will be related to their position on the genetic map. Guerola *et al.* tested the hypothesis that if mutation induced by nitrosoguanidine occurs selectively in the replicating region, then the multiple mutants produced will be closely linked. They examined azide resistant mutants of *E. coli* for the presence of other mutations and found a large excess of these among closely linked genes. They have eliminated the possibility that this increase is a property of the genes themselves.

Because a high proportion of all nitrosoguanidine induced mutants occur within the replication region, it is possible to estimate the length of this region as the distance between the two

outermost genes showing an increase in induced mutation frequency. The replication region has also been estimated to be 220 times more sensitive than a similar length of the rest of the chromosome.

Guerola *et al.* claim that a strain carrying a mutation induced by nitrosoguanidine probably carries fifty other base changes within a map distance of less than two minutes. If so, it is obvious that a phenotype obtained after nitrosoguanidine treatment cannot be the consequence of a single mutational event.

Treatment with nitrosoguanidine may provide a useful tool for genetic mapping in asexual organisms. In *E. coli*, coarse mapping can be achieved by sequential mutagenesis and fine mapping by an index derived from the ratio of the frequency of double mutation to the product of the frequencies of single mutations in these genes. This index is greater than one for markers closer than the length of the replicating region. Nitrosoguanidine mutation also has possibilities as a tool for isolating mutants. By selecting for mutations to a closely linked gene, relatively high frequencies of inactivating mutations may be expected. Because there are no special features in the chemical structure of nitrosoguanidine which can explain its affinity for the replicating region, it would be of interest if other mutagens could be found with a similar affinity.

PROTEINS

Laws of Resurrection

from our Molecular Biology Correspondent

THE manner in which a coiled polypeptide chain finds its way through an infinitude of alternatives into the unique structural state of a native globular protein is a subject that has tormented and mesmerized protein chemists for many a year now. This week sees the appearance of some interesting new results and new cogitations. Teipel and Koshland (*Biochemistry*, **10**, 792; 1971) have examined the renaturation kinetics of six enzymes from the fully random state in 6M guanidine hydrochloride. All are large subunit proteins, and none has disulphide bonds. In all cases enzymatic activity returns when the denaturant is diluted out, though in some cases at a rather low level. When ligands, which may be substrates or co-factors, are present the revival of activity is in all cases increased. This means that the ligand, which is, of course, specific for the native conformation, helps to trap the protein in this state. In some cases, however, the ligand must be present from the outset to have any effect, whereas in others its presence alone, even if it is introduced at a late stage of the refolding reaction, determines the level of renaturation. One may deduce that in the first group correct refolding competes with conversion to relatively stable incorrectly folded states. In the other group it seems as though no long-lived states, other than the native and the fully random, need be considered just as in many small proteins.

The data in these experiments are not complete enough, and the extent of reversibility in any case too low, to permit of a more detailed analysis. That it is possible in principle, and often in practice, to discriminate between different causes of divergence from simple two-state behaviour, following first-order kinetics, is incisively demonstrated by Ikai and Tanford in last week's *Nature* (p. 100). They examine the consequences on the one hand of stable intermediates on the refolding pathway, and on the other of side-reactions leading to incorrectly folded species, and show that these have different effects on the unfolding and refolding kinetics. The interesting case, which seems to relate to some experimentally documented situations, and is consistent with the conjectures of Teipel and Koshland, involves the presence of a side-reaction leading to a conformational blind alley, from which the system must reverse through the unfolded state in order to reach the native structure. Ikai and Tanford and also Teipel and Koshland envisage false starts in the initial stages of refolding.

Teipel and Koshland find that the dependence of the reactivation efficiency on protein concentration is startlingly variable. Because all the enzymes examined have a quaternary structure one will expect self-association, which will be favoured by high concentrations, to effect the trapping of the native state in just the same way as ligand binding. Aggregation of the denatured form is also a common occurrence, however, and the overall result is presumably determined by which effect wins. What remains unexplained is why the renaturation is always incomplete.

One may note in this connexion a well characterized example of such a phenomenon: Zimmerman and Coleman (*J. Biol. Chem.*, **246**, 309; 1971), in studying the thermal denaturation of pancreatic deoxyribonuclease, which encompasses a much less complete disordering than guanidine hydrochloride, found that if the denatured form is

rapidly cooled it forms a dimer, identifiable by gel filtration. This species has an extremely low dissociation constant, is inactive, and thus functions as an efficient trap for the thermally denatured state, which otherwise returns rapidly to the native structure.

In their second article (*Biochemistry*, **10**, 798; 1971), Teipel and Koshland advert to the change in optical properties on dilution of the denaturant. They find that the changes in both optical rotation and fluorescence towards the values characteristic of the native state are essentially complete within one minute. Revival of enzymatic activity ensues much more slowly, however, indicating that the gross refolding process is not completely tidy. The possibility of local conformational effects, not reflected in the optical properties of the protein, is one which it has not previously seemed necessary to consider.

Interferon and Transformation

IN next Wednesday's *Nature New Biology*, Taylor-Papadimitriou and Stoker describe the effect of interferon, the protein made by cells to prevent the replication of invading viruses, on the abortive transformation of hamster cells infected with the DNA tumour virus, polyoma virus. They believe their results show that abortive as well as stable transformation depends on the expression of at least part of the polyoma virus genome.

When a population of BHK21 hamster cells is exposed to polyoma virus most if not all of the cells are stimulated to multiply even though they are maintained in conditions in which uninfected control cells fail to divide. The infected cells grow into small colonies of up to thirty-two or sixty-four cells and then most of them cease to multiply further. The majority of cells, which stop dividing at this stage, are described as being abortively transformed, and the few infected cells—about 5 per cent—which continue dividing indefinitely are described as being stably transformed. The transforming viral genomes are integrated in one or more of the chromosomes of stably transformed cells and at least some of the viral genes are continually expressed. The relationship between the virus and the cells which are only abortively transformed is, however, far more obscure and clearly it is more difficult to investigate because of its transience. Pressing questions such as which, if any, of the viral genes are expressed during abortive transformation, the fate of the viral genome and why only such a small percentage of infected, non-permissive cells become stably transformed, pose formidable technical problems.

In an attempt to shed light on the first of these problems, Taylor-Papadimitriou and Stoker examined the effect of hamster interferon on the course of events following the infection of BHK cells by polyoma virus. In brief they found that by exposing the cells to interferon before and 3 hours after infection stable transformation is blocked, abortive transformation, as judged by the orientation of the cells and their failure to grow in suspension, is blocked and the induction by the virus of cellular DNA synthesis is blocked. This last result is, of course, reminiscent of Dulbecco and Johnson's finding that interferon prevents the induction of cellular DNA synthesis in permissive cells supporting the complete replication of polyoma virus and destined to die as a result.

Valid interpretation of these results hinges on understanding how interferon acts and that, it must be admitted, remains in dispute. It seems likely, however, that interferon somehow prevents the expression of viral genes in an infected cell. In which case the observation that both stable and abortive transformation is blocked by interferon must mean that both these responses depend on the expression of at least part of the polyoma virus genome. It has been repeatedly observed, however, that interferon is without effect on stably transformed cells once they are transformed, even though the maintenance of the transformed state apparently depends on the continued expression of the transforming viral genome. Why the initiation but not the maintenance of transformation is sensitive to interferon remains, for the time being, entirely in the realms of speculation.

PHOTOTAXIS

Jumping to Conclusions

ALL frogs may look before they leap, but it is now clear that all anuran species are not alike in their phototactic preferences. Although since the turn of the century it has been widely believed that frogs preferentially face and move towards blue light, Robert Jaeger of the University of Maryland and Jack Hailman of the University of Wisconsin report on page 189 of this issue of *Nature* a new kind of anuran phototactic behaviour; the tropical frog *Eleutherodactylus rhodopsis* does not seem to be sensitive to any particular colour of light, but responds only to the apparent brightness of the light source.

The study of colour vision in frogs has fascinated zoologists chiefly for two reasons. First, the mechanism by which anurans recognize and respond to blue light is an example of very primitive colour vision; it serves as a model which may help to explain the more highly developed systems found in other animals. Second, it is hard to see what benefit this limited colour sense confers on the species—and similarly in what way the lack of colour response may penalize *E. rhodopsis*, if at all.

The principal problem in devising experiments to investigate colour preference in a phototactic response is to ensure that what is measured is really a response to the colour of the light stimulus, that is, true colour vision, and not merely to the intensity of the light used. The test used by Jaeger and Hailman was similar to that designed originally by W. R. A. Muntz, a pioneer in the study of colour vision in frogs. The amphibians were placed singly in a black box facing two screens towards which the animal could jump. The screens were illuminated with different colours and intensities of light and the frogs were allowed to choose which of the screens to jump towards (being helped on their way by a friendly prod with a blunt instrument).

The use of appropriate combinations of filters and light intensities made it statistically possible to show that the leopard frog *Rana pipiens* shows a true colour preference in leaping predominantly towards blue light, whereas the tropical frog *E. rhodopsis* shows no such discrimination, but responds only to the apparent brightness of the source.

Neither the physiological nor the functional significance of this difference in the phototactic behaviour of the two species seems clear at present, but Jaeger and Hailman hope that a more extensive comparative survey of phototaxis in anuran amphibians will clarify the picture. The early work of Muntz (*J. Neurophysiol.*, 25, 712; 1962, for example) clearly established that in ranid frogs, there are specific optic nerve fibres which

run from the retina to that part of the brain called the dorsal thalamus which are responsible wholly for the detection of the onset of illumination and for distinguishing between blue and other spectral colours. Other optic fibres, concerned with movement of small and large objects, run to the optic tectum. In man, where colour vision is highly developed, the tectal network is comparatively unimportant, whereas most of the optic fibres run to the dorsal thalamus, and thence to the cerebral cortex. It will be

interesting to see whether there are significant differences in the structure of the optic nerve system in the brain of *E. rhodopsis* compared with *R. pipiens*.

On the other hand, Muntz has suggested that the blue light response in frogs is a defence mechanism whereby a badly frightened frog can instantly recognize and leap towards the sanctuary of water; it will be equally interesting to see whether similar ecological considerations underlie the lack of response to colour in the tropical frog.

No Polyoma Repressor

DRAWING analogies between the lysogenization of *Escherichia coli* by temperate phages such as lambda and the transformation of cultivated animal cells by the small DNA tumour viruses SV40 and polyoma virus is a fashionable exercise. And it is useful so long as it is not taken too seriously, for in many respects the two phenomena are not closely analogous. If anybody doubts that, he should read the account by Basilico and Wang, in next Wednesday's *Nature New Biology*, of experiments which rule out the idea that a dominant and diffusible immunity factor, which specifically represses the polyoma virus genome, exists in polyoma transformed cells. By contrast, of course, the lysogeny of *E. coli* by lambda phage involves the synthesis of just such an immunity factor; it not only prevents the complete transcription of the infecting phage genome responsible for lysogenization but also renders the lysogenic cell resistant or immune to superinfection.

Lysogeny occurs when bacterial cells are infected by a phage which has the potential to replicate completely in those bacteria and as a result kill them; it is not known precisely which factors determine whether a particular infection will result in lysogeny rather than replication and lysis. Transformation, on the other hand, can occur either when wild type virus infects a type of cell which cannot support the complete replication of the virus, or when a defective virus, inherently incapable of complete replication, infects a permissive cell which can support the complete replication of wild type virus.

A priori it might be anticipated that lysogeny should, as it does, depend on some repressor factor, which somehow prevents the complete expression of the phage genome. If that were not the case, every infection should result in the replication of the virus and death of its host. This argument does not, however, apply to transformation, an event which involves either a defective viral genome in a permissive cellular environment or a functionally complete viral genome in a non-permissive environment, in

which the genome is incapable of full expression.

It has been shown repeatedly that permissive mouse cells transformed by polyoma virus are not resistant to superinfection with wild type virus. This observation is precisely that anticipated. Two French groups have, however, recently reported experiments which, they believe, show that cells transformed by polyoma virus or simian virus 40 contain a "repressor" which specifically inhibits viral multiplication. But not every, indeed very few, tumour virologists were convinced by this claim and Basilico and Wang's results add another nail to its coffin.

These workers made hybrids by fusing non-permissive hamster (BHK) cells, transformed by polyoma virus with uninfected 3T3 mouse cells which support the multiplication of polyoma virus. So long as the hybrid cells retain the hamster chromosomes carrying the integrated transforming polyoma genomes they should also contain any putative repressor or immunity factor and as a result resist superinfection. In fact, however, the hybrid cells, with a slight excess of hamster cell chromosomes compared with the normal complement, were susceptible to superinfection. In superinfected populations of hybrid cells, the frequency of virus-producing cells and cells dying as a result of polyoma multiplication was the same as that in populations of normal 3T3 cells infected with the virus.

The only way to reconcile these observations with the notion that the transformed cells contain a diffusible "repressor" is to postulate further that 3T3 cells contain a dominant and diffusible "antirepressor"; such flights of fancy are completely unwarranted in the face of the much simpler hypothesis that a repressor does not exist. In short, transformation and lysogeny are analogous in so far as both phenomena involve the integration of a viral genome into a host cell chromosome. But the ways in which the expression of the viral genome is blocked in the two situations are by no means analogous apparently.

Scientific Information by Computer

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An evaluation of the Chemical Society's Chemical Titles information retrieval service compared with a parallel manual search of the literature has shown that, at least in certain disciplines, more than 90 per cent of the desired information can be obtained. This is quite adequate for most commercial and scientific purposes.

IN spite of the recent proliferation of computer-based information retrieval systems, little is known of their operating characteristics. It is important to know how fast, how completely, and how precisely the desired information is retrieved, how good a current-awareness service is obtained, and how frequently mistakes occur. Most research on information retrieval has been conducted on small, well prepared, and well characterized sets of documents¹, and results are not necessarily applicable to the bigger, commercial systems such as those based on the *Science Citation Index*, or the Chemical Society's Chemical Titles (CT) service. (This service was first operated in Britain by the Chemical Society's Research Unit in Information Dissemination and Retrieval, Nottingham. It is now run by UK Chemical Information Services, Nottingham.) From the point of view of their commercial and technical future and usefulness to the scientific community it is important that the efficiency of the retrieval methods used in the big systems should be studied in detail^{2,3}. The Office of Scientific and Technical Information (OSTI) is engaged in studies of this kind, monitoring about 200 current-awareness search profiles for 12–16 weeks each. We report here an investigation in which we compared the performance of one carefully constructed search profile for twelve months with a manual search of the relevant literature. We consider that studies on this kind of time scale are essential for a proper appraisal of the search methods used. Our search covered items published in 1968, but it was necessary to extend the experiment well into 1969 to take account of belated publication of some journals and the inevitable time lag before the appearance of abstracting periodicals.

The information we desired was fairly typical of the requirements of many research groups. Our work is concerned with the development of field-ion microscopy and its application to the study of the initial oxidation of metals. Thus our sphere of interest consists of several overlapping regions: (a) developments in a group of related experimental techniques; (b) the study of certain physical effects, and (c) background information about a class of systems in which these effects are important.

Method of Search

The principles of the search methods used by the Chemical Society Research Unit have been described in detail else-

where⁴. The user constructs a list of words and phrases (search terms) that summarize (profile) his information requirements. The issues of "Chemical Titles" are then searched by the computer for the occurrence of terms in the profile, and the relevant items (title, author and journal reference) are printed out. The search profile may be quite complex; authors' names may be used as search terms, as may certain word fragments and journal abbreviations. The terms may be arranged in related groups (parameters) and permutations so that an item is retrieved only if terms from more than one parameter are present. Terms can also be given a numerical weighting, so that only items which score more than a certain number of points are extracted.

Our profile consisted of four parameters, three describing our three regions of interest (containing 34, 13 and 10 terms respectively), and the fourth consisting of a list of authors whose work we considered particularly important (90 terms). All items by important authors and all items in region (a) (experimental techniques) were retrieved automatically, the requirements here being well defined. The requirements in regions (b) and (c) (physical processes and systems of interest) were less well characterized, and demanded a weighting system in which one term from each of these parameters had to be present to effect retrieval. A full listing of the subject profile is given in Table 1.

We paid special attention to one particular technique of profile writing, which we believe was largely responsible for the subsequent success of the search routine. We shall therefore describe it in some detail. Choosing words or phrases to define our requirements, we made maximum use of our existing store of "important" information—our card index system, which we had kept up to date by manual methods for several years. We extracted cards from the previous calendar year (some 166 entries) and, on the basis of the title and author information on the cards, constructed a profile that would retrieve virtually every entry (apart from one or two completely anomalous ones). We compared this profile with some of our earlier attempts, which had been based simply on general knowledge and memory. We found that our ideas of how people in our field of research wrote their titles had been defective; some combinations did not occur as often as we had expected, while others occurred much more often. Also we had overlooked several extremely important word-complexes without which our profile would have been highly defective. We shall discuss some of the implications of these observations later in this article.

The manual search began with the scanning of current-awareness journals such as *Current Contents*, and ending with coverage of the abstracting journals, particularly *Chemical Abstracts* and *Physical Abstracts*, searching by means of both the index and the relevant section headings. This work occupied 4–5 man-hours per week throughout the year. The search was carried out in the Radcliffe Science Library, Oxford, and the facilities of the National Lending Library, Boston Spa, Lincolnshire, were also used as required.

Table 1 Subject Profile which We Used

First Parameter	Second Parameter	Third Parameter
FIM	*Sor*	Tungst*
F.I.M.	Clean*	Irid*
FEM	Surface*	Iron
F.E.M.	Film*	Ferr*
F.E.E.M.	Whisker*	Nickel*
LEED	Single crystal*	Copper
L.E.E.D.	Plane*	Cupr*
RHEED	Face*	Zircon*
R.H.E.E.D.	Oxygen	Rhen*
S.E.S.	Oxid*	Transition metal*
Field ion*	Hydrogen	
Ion microscop*	Nitrogen	
Ion emission microscop*	Carbon monoxide	
Ion project*		
Field evap*		
Field desor*		
Pulse desor*		
Field corro*		
Atom probe*		
Field emi*		
Fowler-Nordheim		
Fowler Nordheim		
Secondary electron spect*		
Auger electron spectr*		
Time of flight		
Time-of flight		
T.O.F.		
Point cathode*		
Cathode point*		
Diffraction of slow electron*		
Diffraction of low-energy electron*		
Diffraction of low energy electron*		
Flash filament		
Flash desor*		

All items containing terms from the first parameter were retrieved. One term from each of the second and third parameters had to be present for the retrieval of other items.

Assessment of Information

We shall describe briefly how we assessed the significance of each item of output from the computer. We used three categories: (i) "irrelevant", (ii) "relevant" and (iii) "important". Category (i) is self-explanatory, (ii) we define thus: that, on the basis of the information supplied by the computer (title, authors, journal reference), there is a *prima facie* case for reading the original document. The "important" papers (iii) are those which, after scrutiny, merit a permanent entry in our card index system.

The exact definition of (ii) is extremely difficult. Acceptance or rejection of a particular item is often based primarily on background information, rather than on information actually contained in the computer output. For example, work done by certain experimental techniques, or on certain systems, tends to be rejected out of hand, while work

done using methods or systems totally unfamiliar to the searcher may be investigated further, only to be rejected later. We endeavoured to make consistent judgments of the "relevance" of the computer output, putting aside our outside experience, but we found it impossible on the scale required for the manual search, because of the excessive time and effort involved. (The manual search covered about 130,000 items (not counting duplications in abstracts journals, current-awareness journals and so on), whereas the total computer output was less than 2,000 items. It seemed pointless to ask "Would this have seemed important if we did not know better?" for all 130,000 documents, although it was feasible to do this for the computer output.) Thus we have been able to compile statistics of the "relevant" and "important" items of computer output, but only the "important" manually retrieved items. In the next section, we use the "relevant" items to evaluate the precision of the computer search, but only the "important" items to evaluate the other parameters—accuracy, completeness, and speed of retrieval.

Quality of Retrieval

The main results of manual and computer searches are summarized in Table 1. Because the computer profile was not operational in time to search the first three (fortnightly) issues of CT for 1968, we searched these by hand, noting only the "important" items; these have been included in Fig. 1. The principal feature of Table 1 is the high degree of completeness of retrieval, about 90 per cent. This has enabled us to study several aspects of the system in more detail than usual. However, this result has been achieved only at some cost to the precision of the search.

We calculate the precision of the search in terms of the proportion of "relevant" items in the computer output from the twenty-three issues of CT searched in 1968. This is not the same basis as items dated 1968, for some 1967 items appeared in the 1968 computer output, and several 1968 items in the 1969 output. The average result, however, should be the same. A total of 1,783 items were retrieved by the computer (seventy-eight per issue), of which 200 (nine per issue) were considered "relevant". This gives a precision of 11 per cent, which is low by the normally accepted standards (private communication from P. Leggate, Oxford Experimental Information Unit) of automated information retrieval (20–50 per cent). Furthermore, the profile retrieved some 1.5 per cent of all items listed in CT (130,000), which is again somewhat beyond accepted standards. To us, as potential users of the service for our everyday research needs, however, accuracy and completeness of retrieval were paramount considerations, and the normal level of retrieval completeness (40–60 per cent, private communication from

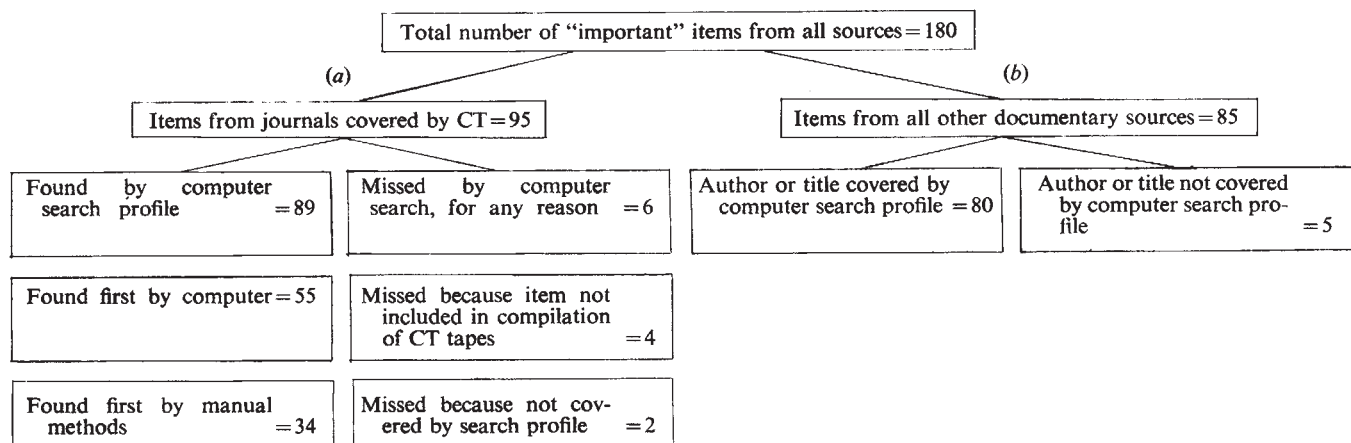


Fig. 1 Analysis of all "important" references from calendar year 1968.

P. Leggatte) was considerably below our requirements. The only initial boundary condition that we placed on precision was that the time taken to scan the computer output should be tolerably brief. In practice, a scan time of about 15 minutes per fortnight was required, compared with the 4–5 hours per week (minimum) needed to search the relevant literature by hand. Thus we consider that our initial boundary condition has been achieved, and that we have effectively transferred the burden of effort from man to machine. In more recent work we have been able to improve the precision of our search procedures considerably, basing our refinements on a detailed analysis of the occurrence of the search terms used in our original profile. Further improvements have also resulted from the introduction of cited author and cited reference retrieval methods, on another computer system. We have not yet tried titles-plus-keyword systems.

In the computer search, eighty-nine "important" items were retrieved, out of a possible ninety-five, giving an operating completeness of 94 per cent. Even this is an artificially low value for the efficiency of our profile, for an investigation of the missing six items showed that four of them had never been included in the CT tapes, even though the journals concerned were nominally covered by that service. Two of these would have been retrieved by our profile, had they been included in the CT listing, and therefore a better figure for the completeness of retrieval of our profile would be 91 out of 95, or 96 per cent. The remaining four failures were all due to trivial programming errors, connected with technical phrases in one search parameter. Thus it would have been possible to write a profile to retrieve 100 per cent of our "important" references from the CT tapes, without further detectable loss of precision.

We found five items out of the eighty-five retrieved manually that could not be retrieved by our profile without major alterations. There was one further trivial programming error, but in this case the item was retrieved by means of the author's name.

The most realistic assessment of the completeness of our profile is obtained by taking all the "important" items from both manual and computer searches, and comparing them against the profile, assuming 100 per cent listing on the computer tapes. We have done this both for our existing subject-plus-author profile and for the equivalent subject-only profile (Table 2). Our operating completeness was 95 per cent, with an upper limit possibility of 97.5 per cent, assuming that all trivial programming mistakes were eliminated. The equivalent results for a subject-only profile were 89 per cent actual retrieved and 93 per cent possible. We shall analyse the causes of failure later.

It is difficult to put any quantitative figures on the relative speed of retrieval of the manual computer searches, because of factors such as delays in the postal service, handling times in the library, the effect of holidays, and the pressure of other work. The only useful statistic is that shown in Table 1—namely, that of the eighty-nine items retrieved by the computer search, the computer notified us first on fifty-five occasions, and the other thirty-four items we found first by hand. Even this figure does not fully represent the facts, because the principal factor governing speed of retrieval was

geographic; English journals were mostly received very quickly by the library, while title pages of journals from the rest of the world could be flown to America and processed by CT, the tapes returned to the Chemical Society in England, searched, and the relevant items delivered to us before the original journal had reached our library.

Four items in journals nominally covered by CT failed to appear on the tapes. One major paper was missed because, so far as we have been able to ascertain, a complete issue of the *Journal of Chemical Physics* failed to reach the computer tapes. The other three omissions were short items—notes or letters—which seem to have been left out as a matter of deliberate editorial policy when transcribing the rest of the issue. The titles revealed little apparent connexion with chemistry, but the subject matter was germane to our interests. The journals concerned—*Review of Scientific Instruments*, *Trans. Amer. Inst. Min. Engrs, Metallurg. Soc.*, and *Die Naturwissenschaften*—could hardly be described as of fringe interest to chemists. It is clear that the editors of CT were at this time exercising much stricter control on the inclusion of notes, etc., than on full papers; we have retrieved full papers of virtually identical title to those of the notes that were omitted. Although we appreciate the motivation behind this policy, we must challenge the logic of it, for three reasons. First, the editors cannot know their customers' requirements exactly. Second, many short notes and letters contain the first "rush" announcement of extremely important results—just what a current-awareness service should aim to retrieve. Third, the policy forces people to continue scanning numerous publications by hand, when the aim is that the computer should do the work. Therefore we strongly recommend that, except in a few specified cases, journals should be covered either completely or not at all.

Table 2 shows that our profile failed to retrieve only nine items out of 180. In the absence of a list of authors, this number would have increased from nine to twenty. There is no value in a discussion of author retrieval failures, for our boundary conditions on the list of authors were of necessity somewhat arbitrary. But it is interesting to analyse the twenty subject-retrieval failures.

We have defined, in connexion with Table 2, the two principal classes of failure—trivial programming errors, of which there were seven, and serious profile deficiencies, of which there were thirteen. About the former, very little need be said. In two cases hyphenated forms of certain word-pairs ("field-ion" and "field-emission") had been omitted and in the remaining five cases certain variants of phrases ("field electron emission" and "low energy electron diffraction") had been overlooked.

Deficiency

With regard to serious profile deficiency, we found four principal kinds of failure. First, there were seven cases in which the title was specific but incomplete. For example, "Adsorption of Oxygen on Platinum"—this topic was at the fringe of our sphere of interest, but the fact (not stated in the title) that it was being studied using experimental techniques identical to ours made it important to us. Another example was "Adsorption Studies Based on Thermionic Emission"—here the experimental technique concerned was not of great interest to us, but the nature of the system being studied (not stated in the title) made the paper important. Second, there were three cases in which the title was too general. For example, "Physisorption on Heterogeneous Surfaces"; it is difficult to see how to retrieve this without further significant loss of precision in the search profile. Third, there was one "pop" title, "Counting Atoms", which made the paper impossible to retrieve by title alone. Fourth, there were two cases of failure due to technical subjects. For example, "Universal Motion Device for Use in Ultra-high Vacuum", a paper describing a technical matter of

Table 2 Calculated Performance of Search Profile assuming 100 Per Cent Listing of Items by Computer

	Using "author + subject" profile	Using "subject only" profile
Total number of items	180	180
Total number retrieved	171 (95%)	160 (89%)
Failures:		
Trivial programming errors	9	20
Serious profile deficiencies	4	7
Maximum retrieval from corrected profile	175 (97.5%)	167 (93%)

considerable importance to us from a practical point of view, but having no necessary specific connexion with our research topic. It is extremely difficult to cover this by automatic retrieval methods.

Unlike many information scientists, we set out in this work to determine the limits of performance of a large, commercial, computer-based information service, and have paid scant regard to search precision in doing so. The statistical significance of our work is open to some question; our results obviously cover only a tiny part of the overall search field and probably depend to some extent on the particular nature of the information required. But we hope to have set some standards of rigour in this kind of investigation, and trust that similar work will be pursued vigorously elsewhere.

The most salient feature of our work is that, for a service based on title-plus-author information only, the upper limit of performance attainable by the system is only just about level with the lower limit of acceptability to the serious user, who wants access to virtually all important facts as and when they become available. A combination of inevitable small profile errors, limitations in profile coverage, poor titling of papers, and computer compilation difficulties means that to achieve retrieval efficiencies in excess of 90 per cent involves almost superhuman efforts. We were helped by having a very extensive collection of "important" items from previous years, on which to base our search profile. Also, the volume of our computer output was quite modest, so a relatively low level of search precision was acceptable. In the more general case, where the search background is not so good and the total amount of information required is somewhat indeterminate, it is clear that much more attention must be paid to the detailed construction of the search profile than is customary at present.

Maximum use must be made of existing sources of information to compile a thorough word-list in the relevant area. This is conventional practice, but the need for extreme thoroughness is perhaps not sufficiently stressed*. And then it is necessary to explore in detail the way in which the information is stored in the computer. This is usually done by consulting computer-compiled thesauruses, in which are

analysed all the significant words, phrases and even word-elements held in the computer store, together with the frequency of their occurrence. There is much scope for expansion in this aspect of profile writing, especially in view of the recent introduction of conversational software, and small video display terminals. It is now possible, in principle, for the user to interrogate the computer store directly, inquiring whether certain search terms are present, how often they occur, and even obtaining examples of each term in its various contexts. In this way, the inquirer can swiftly optimize the performance of his draft profile, gearing his search to the *actual style* of the stored data, rather than to what he *imagines* it to be. We have found this distinction to be most important in practice.

The problem is not very different from that of looking up the index of a book; it is very difficult to specify exactly what words you are going to look up, until you have actually seen the style and format of the index. We see the principal editorial role of the computer bureau as the provision of a thorough cross-referencing system, so that the inquirer can be guided swiftly to the appropriate search area. Here again, there is scope for conversational software writing. If these guidelines are followed, we believe it should be possible to attain retrievals of 85–90 per cent of technical papers, even on the basis of title information alone. This is acceptable for most commercial and scientific purposes—in marked contrast to the 40–60 per cent retrieval commonly effected, using current methods of profile construction⁵.

We thank Dr T. H. Cannon for introducing us to mechanized information retrieval, and the staff of the Oxford Experimental Information Unit for their assistance.

¹ Cleverdon, C., Miles, J., and Keen, M., "Factors Determining the Performance of Indexing Systems," ASLIB Cranfield Research Project, Cranfield, 1966.

² Lancaster, F. W., *Amer. Doc.*, **20**, 119 (1969).

³ Abbott, M. T. J., Hunter, P. S., and Simkins, M. A., *ASLIB Proc.*, **20**, 129 (1968).

⁴ Kent, A. K., *Chem. Ind.*, 1214 (1968).

* *Note added in proof.* Our attention has been drawn to similar (though less successful) use of this technique by J. K. Barkla (*Information Scientist*, **3**, No. 1, 13 (1969)).

Gaseous Atmospheres of Comets

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A year ago the Orbiting Astronomical Observatory detected vast atmospheres of hydrogen around the two bright comets of 1970. In this article Professor Biermann describes some of the events leading up to the discovery.

THE discovery by space vehicles of huge hydrogen atmospheres around the heads of the two bright comets of 1970 shows that hydrogen atoms are released in amounts many times larger than those of the visible molecules which make up the head of a comet as seen from the ground. This means not only that the primary gas must contain large amounts of molecules rich in hydrogen, such as water or hydrocarbons (which are invisible from the ground), as has

been widely accepted for some time, but also that the gas output of a comet must be many times larger than indicated by the molecules normally observed. This latter proposition has been discussed at least since 1964, and supporting independent evidence has come up almost every year since then (see the report of IAU Commission 15 given at the General Assembly, Brighton, 1970). The first quantitative estimates of the properties to be expected of the hydrogen atmospheres of comets had in fact already been given in 1968.

In this article I shall describe the recent observational and theoretical developments in our understanding of comets which are directly related to the present discovery. I shall also give a tentative interpretation, essentially along the lines of the earlier work, and outline some further lines of research.

The emissions from comets at optical wavelengths indicate that the production rates of the most common neutral or

ionized molecules, in medium bright comets, are up to 10^{28} molecules per second. It has long been known, however, that selection effects, which determine the presence of observable emissions at wavelengths above 3000 Å, exclude all those stable molecules which might be present as ices in the nucleus. As a consequence even the order of magnitude of the true gas output of a comet was unknown until recently; only estimates could be given based on plausible ratios of the output of gas and dust particles (lower limits to the production rate of dust were available, for example $\approx 10^8$ g s⁻¹ for comet Arend-Roland¹).

This situation began to change with the proof that the red line at 6300 Å in the night sky, which arises from the lowest forbidden transition of atomic oxygen and which has often been seen near comets, is in fact produced to some degree in almost all bright comets²⁻⁴. The difficulty of proving this is connected with the fact that there is always a high probability of a chance coincidence with a local brightening of the night sky emission. Only spectra with high dispersions can separate the two emissions by the Doppler effect (of usually several tens of kilometres per second); such spectra require fairly large instruments because of the rather low surface brightness of comets. In fact it turned out that the green line in the night sky, the presence of which in the spectra of some comets had also been suspected, is never present in comets in measurable intensity. This line arises from a metastable state 4.2 eV above the ground state of atomic oxygen, whereas the upper level of the red line is only 2.0 eV above the ground state.

The subsequent discussion of the possible mechanisms leading to the emission of the red oxygen line revealed that it was most probably connected with the production of the oxygen atoms by dissociation of molecules such as OH (which is observed), H₂O, or CO₂. Both collisional and photodissociation are known to lead with some probability to the formation of an excited oxygen atom, whereas excitation by solar light would require the presence of a probably still larger number of O atoms and, in view of the finite lifetime of each atom before ionization, a production rate at least as large. The total number of quanta from this transition should therefore be a measure of the amount of molecules containing oxygen released by the comet⁵. It can be estimated by combining the number of transitions per square centimetre column (which is related to the observed surface brightness and is usually expressed in Rayleighs) with the cross-section of the emitting regions. Although such an estimate is crude, it shows that the production rate, for medium bright naked eye comets, is larger at least by two and possibly by three powers of ten than the production rates typically found for the visible molecules—essentially those which have resonance transitions in the spectral range above the atmospheric cut-off near 3000 Å. In numbers this means rates of 10^{30} – 10^{31} molecules per second, or roughly 100 tons. This latter figure is similar to the rate of release of dust particles which has been estimated for a few comets from the observed properties of dust tails. This result led immediately to the prediction that comets should be conspicuous in the light of Lyman α (1216 Å), the principal resonance line of atomic hydrogen, in the following way.

The general chemical character of a cometary atmosphere suggests a hydrogen-rich rather than an oxygen-rich composition, in line with the generally adopted chemical composition of the original solar nebula, from which the comets should have been formed with the planets 4.5 billion years ago. In this I follow the arguments presented, for instance, by J. Oort in his Halley Lecture for 1951; at about the same time, Whipple¹ developed his icy-conglomerate model in which a typical comet is assumed to possess a solid nucleus made up chiefly of ices of hydrogen-rich compounds of oxygen, carbon, and nitrogen, mixed with dust particles in amounts differing greatly from comet to comet. Most of the observed oxygen might therefore be derived

from ordinary ice. In any case the number of hydrogen atoms should, in this picture, be comparable with that of the oxygen atoms, assuming that hydrocarbons do not outnumber the molecules containing oxygen, which seems unlikely.

The brightness of a comet in the light of Lyman α is then found by combining the production rate with the probable lifetime of a hydrogen atom before ionization ($\approx 10^6$ seconds) and with the probability of excitation by the flux of solar Ly α quanta. Another crude estimate gives a total emission of 10^{33} Ly α quanta by a comet per second, which is similar to the emission in the ordinary optical range by molecules like C₂ and CN.

The first attempt to check the order of magnitude of the rate of gas production found from the brightness of the red oxygen line made use of the values of the cross-sections of cometary nuclei derived from the brightness of the central condensation at large distances from the Sun, where there is not too much interference by dust. The most extensive recent work using this method, which actually yields only the product of the albedo and the effective cross-section, is due to Elizabeth Roemer, who found, for plausible values of the albedo, values of the radius of a few kilometres to more than 10 km. These results, in combination with the production rates, make it immediately clear that the gas cannot have been adsorbed by the solid nucleus (or a group of solid nuclei travelling together), and are thus a strong argument in favour of the icy-conglomerate model proposed by Whipple. At the same time this means that in view of the dust content of the ice, small values of the albedo are more likely than large ones. On this basis the relations known from physical chemistry for the dependence of the vapour pressure on the temperature and the heat of evaporation may be used to check whether reasonable temperatures are found and whether, allowing for the heat radiation from the nucleus, the total production rate of gas varies with solar distance in the same way as the production rates of those constituents which can be observed from the ground. This was done by Huebner in 1965 (ref. 6); within the limits of the uncertainties, he found the same values for the total rate of gas production as had been given by Biermann and Trefftz⁷. (Results similar to those of Huebner were independently arrived at by Delsemme⁸.)

A further, again entirely independent, method was developed in 1967 by Finson and Probst⁹ which leads to a value of the gas production of comet Arend-Roland 1956 which is probably rather accurate. The chief object of the work of Finson and Probst was a detailed theory of the dust tails of comets, probably the most important advance since the work of Bessel and of Bredikhin in the nineteenth century, and the gas production rates were a byproduct found in the following way. The observed properties of dust tails require that the dust particles gain a certain initial velocity with respect to the comet's nucleus, of the order of 10 m s⁻¹, which is ascribed to the drag produced by the gas streaming out from the surface of the nucleus. From the observed value of this velocity the gas output can be derived by standard methods of fluid dynamics. This was done for the dust tail of comet Arend-Roland, which was a rather dusty but otherwise typical medium bright comet and for which particularly accurate photometric data were available; the result was a figure for the gas production close to the middle of the range suggested by Biermann and Trefftz⁵ in 1964. Yet another way of getting information on the total gas density in the innermost parts of the heads of comets is to look for pressure effects in the rotational structure of certain molecular bands. This method, very difficult in practice, was developed by Malaise at Liège⁶, and although certain points of detail may still require further discussion, he seems to have shown convincingly that such effects, chiefly due to the collisions with other neutral molecules, do exist. The figures for the gas density necessary to explain

such effects are in the upper range of the figures derived from the other methods.

When it had become clear, around 1967, that the results of all these methods converge, an attempt seemed justified to predict in more detail the properties of pictures of comets that might be taken in the future in the light of the Lyman α hydrogen line (ref. 10). A production rate of 4×10^{30} hydrogen atoms per second was assumed, and an average speed of outflow of 6 km s^{-1} , which corresponds to an original "temperature" of slightly less than 2,000 K. The outer extent of the hydrogen cloud would be determined by the ionization by solar ultraviolet light and by charge transfer in the solar wind, which combined with the average velocity would give an apparent radius of the order of 10^7 km at 1 AU from the Sun. This is many times larger than the head of a comet as seen from the ground. Over the main part of the cloud inside this radius the surface brightness would decrease in inverse proportion to the distance from the nucleus. Attention was drawn, however, to the fact that the deceleration by the pressure of the solar flux of Ly α quanta should be, in average conditions, $1/3 \text{ cm s}^{-2}$ or about half the value of solar gravity. This has the consequence that the velocity of outflow of the hydrogen atoms in the direction of the Sun should be halved after $\approx 10^6 \text{ s}$, in which a distance of $4\text{--}5 \times 10^6 \text{ km}$ would be reached if second order effects can be neglected. Nearer to the Sun the effect would become apparent at a smaller distance from the nucleus, in proportion to the square of solar distance. The excitation rate, at 1 AU from the Sun and for average conditions, was found to be 10^{-3} s^{-1} . This would mean $\approx 10^{10}$ transitions per square centimetre column per second, or 10 kRayleighs for a ray passing at a lateral distance from the nucleus of $\approx 1.6 \times 10^6 \text{ km}$, or 1 or 2 kRayleighs near the ionization distance. The total number of Ly α transitions corresponds to some $10^{22} \text{ erg s}^{-1}$, or an astronomical magnitude of +2 to +1. In the innermost parts, less than 1 or $2 \times 10^6 \text{ km}$ from the nucleus, self-absorption must become important; the maximum brightness near the centre will be determined by the profile of the solar emission line and the velocity dispersion of the hydrogen atoms, which fixes the fraction of solar Ly α quanta which can be absorbed.

On the basis of this work, preparations for observing future bright comets from stabilized rockets in the light of Ly α were begun at Munich in 1968. They were not yet complete, however, when the first bright comet of the winter 1969/70, Comet Tago-Sato-Kosaka 1969g (Fig. 1), was observed by A. Code, C. Lillie and their co-workers with

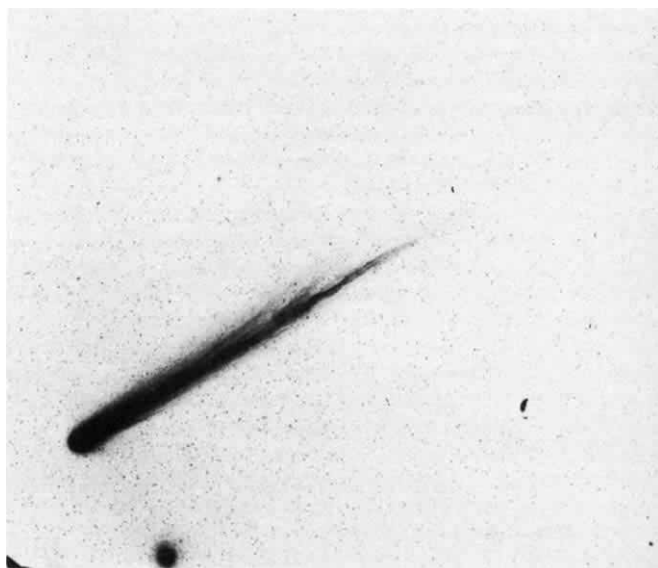


Fig. 1 Comet Tago-Sato-Kosaka 1969g on January 10, 1970 (Boyden Observatory).

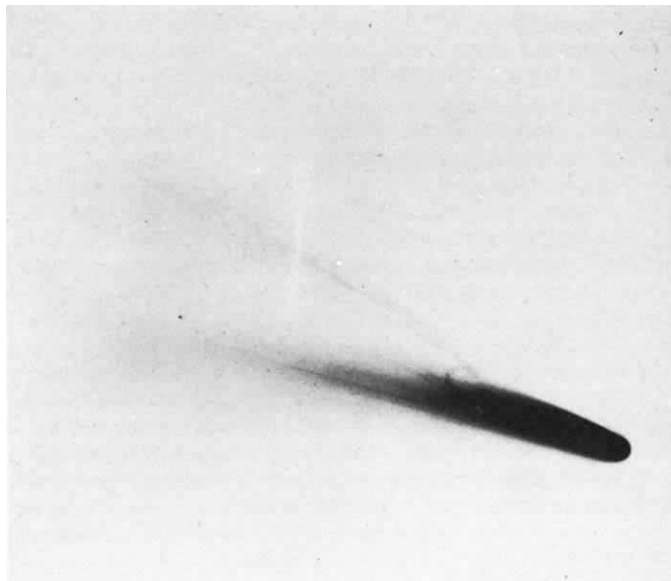


Fig. 2 Comet Bennett 1969i on April 4, 1970 (Sternwarte Hamburg-Bergedorf).

the US Orbiting Astronomical Observatory (OAO 2), which was launched in late 1968. Several spectra of high spatial resolution were obtained which immediately revealed the existence of the hydrogen cloud, the high brightness in the central parts, and its large extent. At the time of the first observation (January 14) the comet had a heliocentric distance of $110 \times 10^6 \text{ km}$, which increased rapidly to about $200 \times 10^6 \text{ km}$ near the end of the observations in mid February. (The perihelion at 0.47 AU had already been passed on December 21; the inclination of the orbit was 76° .) Because of the comet's large orbital velocity the absorption in the (hydrogen) geocorona was usually insignificant. The geocentric distance fortunately had a minimum of $60 \times 10^6 \text{ km}$ around January 28. The observing conditions—including the angular distance from the Sun—were thus excellent, and the comet's astronomical magnitude was about 3 on January 20. The maximum brightness in the centre was found to be $\approx 70 \text{ kRayleighs}$ or $30\text{--}40 \text{ kRayleighs}$, if reduced to a solar distance of 1 AU. The comet was optically thick in Ly α out to $0.5 \times 10^6 \text{ km}$, and the envelope was seen out to about 2° from the nucleus ($\approx 2 \times 10^6 \text{ km}$ at the initial distance from the Earth). From these data it seems that the gas production might have been somewhat smaller, but not by as much as a factor of order ten, than the model comet described here. The Sun showed only moderate activity during the period in question.

In March 1970 Comet Bennett 1969i (Fig. 2) became observable, differing from Tago-Sato-Kosaka by its extreme dustiness and the finite but long period of $\approx 1,700$ years. The extreme dustiness meant that the optical brightness was likely to lead to an overestimate of the gas production when compared with other comets of similar brightness.

Comet Bennett was observed in Ly α not only with OAO 2 but also, on several nights, by Bertaux and Blamont of Paris¹¹ by means of a monochromatic photometer on board the eccentric orbit satellite OGO 5, and less extensively furthermore by Ch. Barth and G. Thomas of the University of Colorado, who used an ultraviolet photometer on board OGO 5 (personal communication). Finally, a stabilized rocket was fired by E. Jenkins and co-workers of Princeton Observatory, which also revealed the existence of a hydrogen atmosphere (personal communication). So far I have seen preliminary results only from the first two experiments mentioned. The solar distance of Comet Bennett increased from $90 \times 10^6 \text{ km}$ on April 1 to $120 \times 10^6 \text{ km}$ on April 16, and to $\approx 150 \times 10^6 \text{ km}$ (1 AU) around the end of the month; the geocentric distance changed from $110 \times 10^6 \text{ km}$ to 150×10^6

km, and to $\approx 200 \times 10^6$ km in the same period of time. (The perihelion, at 0.54 AU, was passed on March 20 and the inclination of the orbit was 90° .) The observing conditions were especially good during the first half of the month.

OAO observations near the perigee of Comet Bennett around March 22 showed the expected absorption of the comet's Ly α quanta by the telluric hydrogen atoms in the geocorona. The change of this absorption with time, effectively a scanning of the comet's emission by 20 mÅ per day (20 mÅ is equal to a Doppler shift of 5 km s^{-1}), yielded an apparent "temperature" of the cometary hydrogen atoms of 1,600 K, or a velocity dispersion of $\approx 6 \text{ km s}^{-1}$, which might be taken to indicate a velocity of outflow of similar magnitude.

The spatial resolution of Code's instrument is about one arc minute or some 10,000 km, so it is clear that the transition from the parts optically thick in Ly α to those which were optically thin could be measured. In the central parts, up to 0.1 of the total solar Ly α flux was absorbed; for the outer parts, the average dependence of the brightness on distance from the nucleus was approximately proportional to the inverse first power as expected.

Before passing to the OAO observations of other molecules, I shall discuss the Ly α results of Bertaux and Blamont obtained on April 1–2 (Fig. 3). The spatial resolution of their instrument was much smaller (only $40'$) but the sensitivity, on the other hand, permitted them to measure the emission of the cometary hydrogen atmosphere down to the limit imposed by the background of ≈ 200 or 300 Rayleighs, which seems to be essentially due to solar quanta reradiated by interstellar hydrogen atoms falling towards the Sun. In consequence the cometary data obtained with this instrument show the rapid decrease of the brightness due to ionization of the hydrogen atoms at distances larger than three or four million kilometres from the nucleus which was predicted by the theory.

By integration of the surface brightness, Bertaux and Blamont also derived a value for the emitting mass of the hydrogen cloud at the time of their observation of 2×10^{13} g, or 1.2×10^{36} hydrogen atoms. As the lifetime of the hydrogen atoms before ionization at that solar distance (0.6 AU) can hardly be more than 10^6 s—a figure which is also equal to the ratio of the probable ionization distance as estimated from the isophotes and the probable velocity of expansion—the production rate becomes $10^{30.1} \text{ s}^{-1}$ (at least), or roughly one third of the rate assumed for the model comet at 1 AU. Because Comet Bennett was intrinsically not particularly bright and, as already noted, extremely dusty, this seems reasonable.

Bertaux and Blamont assume one excitation of a hydrogen atom in 350 s, which means an acceleration away from the Sun of 0.93 cm s^{-2} ; particles emitted with a velocity of

6 km s^{-1} and having a lifetime of $\approx 10^6$ s (in which the comet covers many millions of kilometres) must therefore be strongly affected in their motion. Though detailed calculations have not yet been carried out, it is probably fair to say that the observed flattening of the outer isophotes in the diagrams of Bertaux and Blamont (which were also seen by Code and Lillie) can be ascribed to the light pressure in Ly α . The structure of the inner isophotes which is more complicated is not yet understood.

Molecules could be observed only with the OAO instrument. Code reported at the meeting of Commission 15 during the 1970 IAU General Assembly that OH is by far the most conspicuous molecule, but that H_2 is possibly present. Other molecules such as CN and NH are seen only in the spectral region just above 3000 Å covered by the OAO spectrometer. The OH molecule is particularly interesting in view of the possibility that a large part of the hydrogen is the product of the successive dissociation of ordinary water molecules. Code gives an expression for the density of OH as a function of the distance from the nucleus which, with a velocity of outflow of $\approx 1 \text{ km s}^{-1}$ (from ground-based observations), would lead to a production rate of 2 or 3×10^{29} molecules per second. This is much more than has been assumed up to now on the basis of ground-based spectral photometry which, however, is difficult to carry out because the wavelength of the band in question is close to the ozone limit. On the other hand, the production of OH is smaller by a factor of two or three than half that of hydrogen. Some fraction of the OH will already be dissociated or ionized at the level where the optical depth is ≈ 1 , however, so this discrepancy may not be serious. In any case it seems, from Code's spectra, that OH is more abundant, probably much more so, than NH or CN. Unfortunately no upper limit can be given for CO_2 , CO, or atomic carbon, none of which has been observed. Code notes finally that the ultraviolet spectra of the two comets discussed here were quite similar, particularly with respect to Ly α and the OH emissions. The fact that the dust content of the two comets was very different suggests that dust particles (for instance carrying an ice mantle) are not the primary source of the hydrogen.

The OAO data may still contain other important clues, and the observations of these comets from the ground have not yet been fully analysed. In particular no observation of the red oxygen line has been reported as yet. No firm answer can therefore be given to the questions whether molecules like CO_2 and $(\text{CN})_2$ are really outnumbered by H_2O and hydrocarbons, and perhaps NH_3 , and what the relative abundance of the last mentioned constituent is. Such questions will surely be the subject of studies of future comets if space vehicles are available to observe them.

To sum up: the ultraviolet observations of the two very different bright comets of 1970 indicate at least a substantial relative abundance of molecules containing hydrogen and in particular a large total gas production rate, which had been assumed on various grounds for some time. Beyond that the observations seem to give direct support to some theoretical models, which so far had been based more on inference or deduction than on direct observational evidence.

¹ Whipple, F., *Astrophys. J.*, **111**, 375 (1950); *ibid.*, **113**, 464 (1951); *ibid.*, **121**, 750 (1955).

² Swings, P., and Greenstein, J., *CR Acad. Sci., Paris*, **246**, 511 (1958).

³ Swings, P., and Greenstein, J., *Ann. Astrophys.*, **25**, 165 (1962).

⁴ Remy-Battiau, L., *Ann. Astrophys.*, **25**, 171 (1962).

⁵ Biermann, L., and Trefftz, E., *Z. Astrophys.*, **59**, 1 (1964).

⁶ Huebner, W., *Z. Astrophys.*, **63**, 22 (1965).

⁷ Delsemme, A., *Liège Colloquium*, 1965, 37, 69, 77 (1966).

⁸ Finson, M., and Probst, R., *Astrophys. J.*, **154**, 327, 353 (1968).

⁹ Malaise, D., thesis, Liège (1968).

¹⁰ Biermann, L., *JILA Report No. 93* (1968).

¹¹ Bertaux, J., and Blamont, J., *CR Acad. Sci., Paris*, **270**, 1581 (1970).

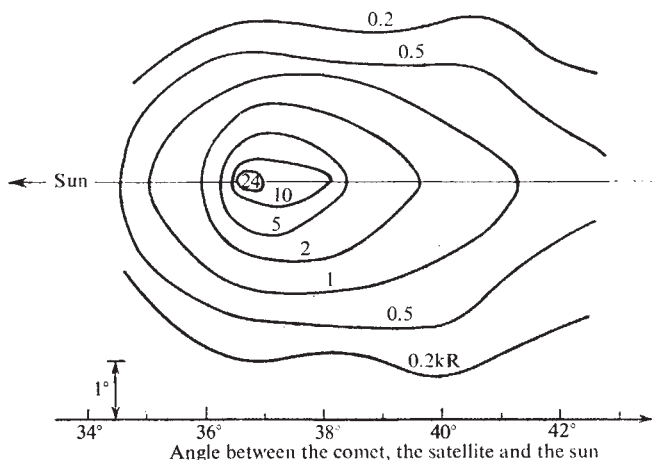


Fig. 3 Isophotes of Lyman α for Comet Bennett 1969i on April 1, 1970, at 1500 GMT (Bertaux and Blamont¹¹).

Acoustic Doppler Measurements of Vertical Velocities in the Atmosphere

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Acoustic echo sounding techniques have been used to determine vertical velocities in the atmosphere during thermal plume activity and a nocturnal inversion. The existence of breaking wave formations within the inversion has been clearly established.

EXPERIMENTS during the past few years^{1,2} have clearly demonstrated the potential of acoustic echo sounding as a means of monitoring atmospheric structure and motions. This work, although striking in detail, does not provide quantitative measurements and the interpretation of the patterns has been based chiefly on speculation about the associated velocity and temperature fields. In this article we describe how Doppler information from acoustic echoes was used to derive vertical velocities both during daytime thermal plumes and during the occurrence of breaking waves within a nocturnal inversion.

The scattering of sound waves by turbulent temperature and wind fluctuations has been described by Tatarski³, Kallistratova⁴, Monin⁵ and others. The potential of this scattering process for remote sensing of atmospheric structure and motion has been fully documented elsewhere^{4,6,7}, but we wish to point out that for the monostatic case (that is, using collocated transmitting and receiving antennas) only temperature fluctuations contribute to the received acoustic echo power.

Doppler Shifts

The radial velocity, v , of a scattering target is given by

$$v = \frac{c}{2} \left(\frac{f_0}{f_d} - 1 \right) \quad (1)$$

where c is the phase velocity of the acoustic wave in the radial direction, f_0 is the original carrier frequency, and f_d is the frequency of the returned signal. Second order velocity terms are neglected in the derivation of equation (1), but although this simplification can readily be made for electromagnetic waves, caution must be exercised when making the same simplification for the acoustic case, especially if very high wind speeds are to be measured. We were measuring vertical velocities less than 2 m s^{-1} so the second order terms are unimportant.

The much slower speed of acoustic waves compared with electromagnetic waves can introduce some errors into the

Doppler measurements. If the medium along the path to the target is varying with time, which is usually the case in the atmosphere, additional frequency shifts may be added to the true Doppler shift produced by the radial motion of the target. Fluctuations in the vertical velocity of the air along the path do not, however, produce spurious frequency shifts, provided that the vertical velocity field does not change between the upward and the downward traverse of the medium. On the other hand, temperature fluctuations, which produce phase changes of equal magnitude and sign in both propagation directions, will not cancel out. This spurious propagation-induced frequency shift should not produce errors in derived vertical velocities of more than about 0.1 m s^{-1} for the heights which concern us.

Table 1 Acoustic Echo Sounder Parameters used during the Doppler Experiment

Peak power	6.4 W (acoustic)
Pulse width	100 ms
Pulse repetition frequency	1 s^{-1}
Maximum range	170 m
Carrier frequency	4,000 Hz
Antenna diameter	122 cm
Antenna beam width	$\pm 2^\circ$ to 3 db points
Beam direction	Vertical
Receiver band width	100 Hz
System noise level in 100 Hz band width	$\sim 10^{-18} \text{ W}$
Scattering volume at 100 m altitude	Cylindrical, $\sim 7 \text{ m}$ diameter by 17 m deep

The frequency spectrum of the returned signal is compared with that of the original transmitted pulse to obtain the Doppler frequency shift. The method of obtaining the spectra differs slightly in the acoustic and electromagnetic cases, primarily because of the characteristically much higher pulse repetition rate used with electromagnetic radar. The final spectrum generated from a radar return is extracted from information contained in each of many separate pulses and it is this spectrum which is used to reproduce the Doppler frequency; in the acoustic case, a spectrum can be generated for each pulse. The higher pulse repetition rate of the radar does not increase the accuracy if the signal-to-noise ratios and overall duty cycles (fraction of time that the transmitter is on) are equal in the two cases. A further improvement can

be made by averaging the acoustic spectra from succeeding pulses, although this reduces the horizontal resolution (by increasing the size of the total volume being considered) because of the ambient air motion during the time required to collect the sample.

Resolution

Vertical resolution is much higher for the acoustic Doppler method because of the difference in the speed of the acoustic and electromagnetic wave. Standard pulse radars have range resolutions of about 100 m, but acoustic sounders readily attain resolutions of the order of 10 m, and can be further improved by using shorter pulse lengths. The vertical resolution is then only limited by the length of the transmitted pulse, suggesting that a very short pulse length would be optimum. The section of optimum pulse length must be a compromise, however, in order to reduce the transmitted band width and to increase the signal-to-noise ratio. In both cases, FM-CW or other broad band modulation techniques can be used to obtain better range resolution if necessary.

The relevant parameters of our equipment are shown in Table 1. The pulse repetition rate was fixed at 1 s^{-1} and gives a maximum usable range of 170 m. The carrier frequency was increased to such a value that echoes were obtained at approximately the maximum range with a minimum of return from higher levels; this ensured that echoes from preceding pulses would not contaminate the returns—the strong dependence of atmospheric attenuation on frequency makes this a suitable method for limiting the range of an acoustic sounder. The antenna beam width becomes narrower at higher frequencies, and so the spatial (horizontal) resolution of the system also improves as the frequency is increased.

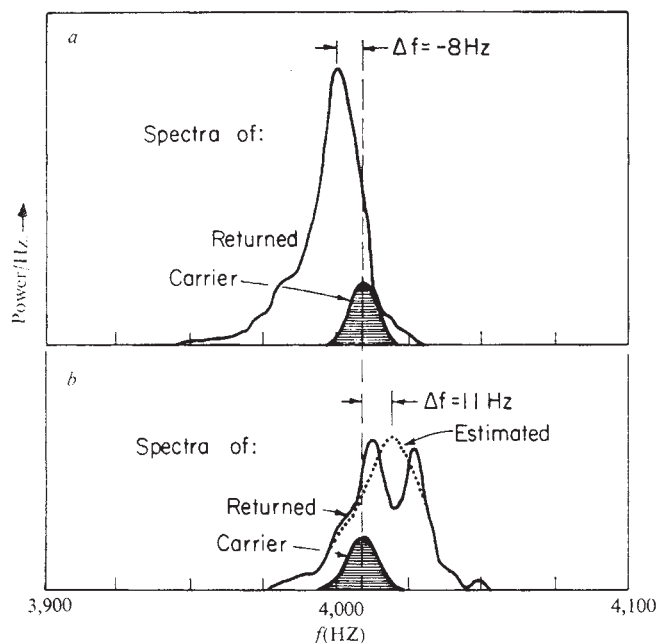


Fig. 1 Example of spectra used to determine the Doppler shift. Amplitudes of spectra are not plotted on the same scale.

The data were reduced in a two channel digital processor ('Time Data-100') which derived the frequency spectra of the returned signal at predetermined range gates corresponding to selected heights. The stability of the estimate was increased by taking spectra from each of ten succeeding pulses and then averaging them together. The power spectra for transmitted pulses and the return echoes were then plotted on a linear scale of frequency against power; the Doppler shift was

determined by visual inspection and it was therefore necessary to limit the number of spectra by using only alternate groups of ten pulses at a given height and alternating the groups of ten from level to level, thus creating a checkerboard pattern of sampled volumes in time and height. The best fit curve through the computed spectrum was estimated and the peak taken as its first moment. The mean vertical velocity of the volume was determined from equation (1). The spectra were fair approximations to a Gaussian curve (see Fig. 1a). A few of the spectra had bimodal shapes (see Fig. 1b) suggesting that two or more separate velocity regions were contained within the volume sampled by the sounder during the 10 s averaging period.

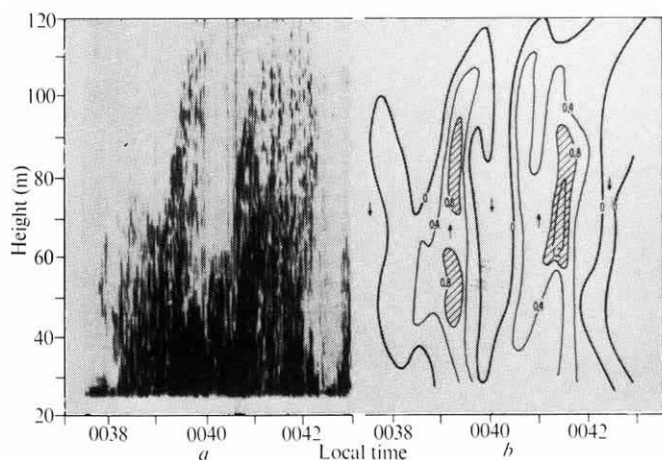


Fig. 2 Acoustic echoes (left) and Doppler derived vertical velocity field during thermal plume activity. Contour interval is 0.4 m s^{-1} ; regions of vertical velocity $< 0.8\text{ m s}^{-1}$ are shaded; arrows indicate direction of vertical component.

Plumes and Inversions

Two cases were analysed which represented distinctly different vertical velocity regimes. The first set of data was taken during the afternoon of October 9, 1970, when convective thermal plume activity was present. A portion of the facsimile record for this time (see Fig. 2a) shows the characteristic strong returns from the ascending air and weaker or non-existent returns from the surrounding region; the derived vertical velocity field is shown in Fig. 2b. The strongest acoustic echoes do not correspond exactly with the regions of maximum vertical velocity, but are displaced slightly in a downwind direction and arrive first at the sounder. This implies that the strongest temperature fluctuations are associated with the region of maximum shear between the rising warm air and the descending cooler air, as would be expected. The vertical or even slightly upwind slope of both the echoes and the vertical velocity field seems to be in disagreement with results of previous thermal plume studies in which it has been shown that the slope of the plume is a function of the vertical shear of the horizontal wind⁸. Conventional wind data associated with the two cases were available only at a height of 5 m, and there is no way to correlate the slope of the plume with actual measured wind shear. Also, because of equipment limitations, the lowest echoes occur at about 25 m and the slope of the plume between this level and the ground—the region where most previous tower measurements have been made—is not available for comparison. For a well mixed atmosphere, which characteristically accompanies thermal plumes, the wind profile would contain strong shear in the lowest few metres, but vertical momentum transfer would tend to eliminate shear at higher levels. This leads to the conclusion that the observed slopes are realistic; the

apparent inconsistency with previous studies seems principally due to the different height ranges investigated.

Data for the second case were taken from acoustic measurements made between 0035 : 40 and 0042 : 00 h on October 12, 1970, just before passage of a cold front. During the time before and including this example, the nocturnal inversion exhibited large oscillations with amplitudes of about 100 m and a period of 15 to 20 min. Fig. 3a shows a portion of the last oscillation before the complete breakdown of the structure, which is accompanied by an abrupt increase in the surface wind velocity from 3 m s^{-1} to 12 m s^{-1} . Smaller oscillations are clearly visible within the larger one and are interpreted as the return from a breaking gravity wave. The Doppler derived vertical velocity field (see Fig. 3b) shows the alternating up and down motion as these smaller waves pass the acoustic sounder with the region of downward velocity ahead of the breaking wave in the centre and top half of the picture. The "spillover" from the breaking gravity wave is seen just before 0039 h at heights between 90 and 100 m.

A slightly different presentation of the breaking wave is shown in Fig. 4; the wind speed at a height of 5 m was used to estimate the horizontal translation of the air. Assuming that the values recorded at this height are representative for all heights, it is possible to interpret a streamline pattern through the breaking wave by using the measured vertical velocity components. Although the assumption of constant wind speed through the layer is rather extreme, the procedure does reverse the order of events from right to left and produces a pattern which is more easily recognized as a breaking wave.

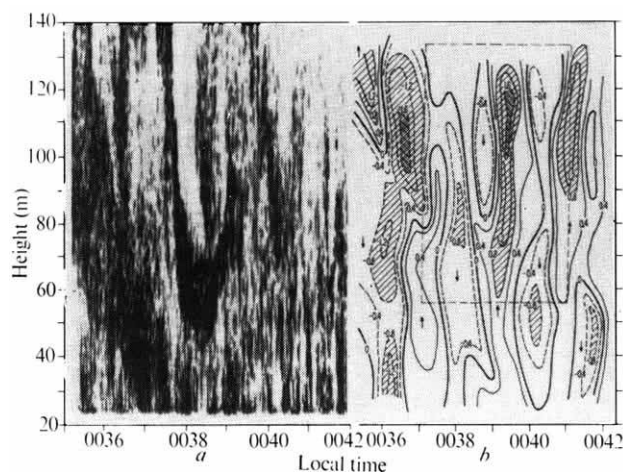


Fig. 3 Acoustic echoes (left) and Doppler derived vertical velocity field under a nocturnal inversion which was exhibiting wave motions. Contour interval is 0.4 m s^{-1} ; velocities $< 0.8 \text{ m s}^{-1}$, single hatching; $< 1.6 \text{ m s}^{-1}$, double hatching; upward vertical velocities, solid lines; downward vertical velocities, dashed lines. Derived streamlines for area within box are shown in Fig. 4.

It is difficult to assess the accuracy of the Doppler velocity measurements because of the method of determining the first moment of the spectra; the remarkable consistency of the velocity field suggests, however, that the accuracy of the velocities is about $\pm 0.2 \text{ m s}^{-1}$. A further accuracy estimate for the velocity measurements can be obtained from the vertical displacement of identifiable portions of the acoustic echoes. The vertical velocity deduced for the descending layer near 0032 h was -1.6 m s^{-1} . This compares with $\sim -1.0 \text{ m s}^{-1}$ measured by the Doppler method, and is in reasonable agreement considering that the Doppler velocities are spatially averaged over 10 s.

Only the first moment of the frequency spectrum was required for determining the Doppler shift but a more complete

analysis of the individual spectra, including the extraction of higher moments, should help to develop a clearer picture of the turbulent structure within a given scattering volume.

Refinements

Other refinements in the technique are, of course, still needed, for example digitization of the acoustic records and on-line data processing. It would also be desirable to have a better understanding of the processes producing spectra which are not normally distributed and which might lead to improved methods of determining the true Doppler shift caused by the vertical motions. A component of the horizontal wind could be measured with improved scanning techniques, including beam tilting.

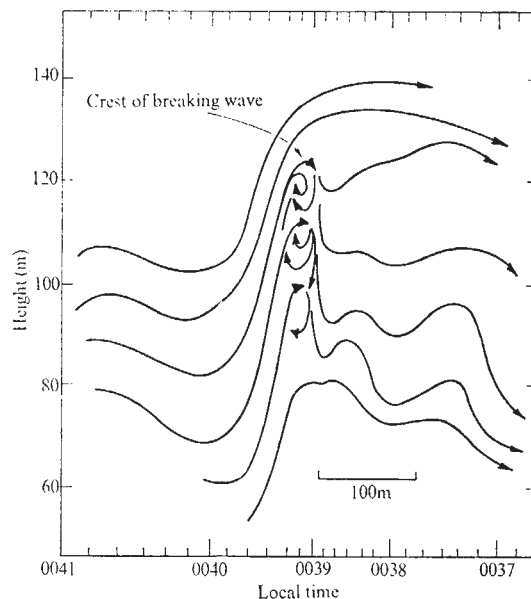


Fig. 4 Derived streamline pattern for region outlined in Fig. 3.

The potential of the acoustic Doppler approach as a method of measuring velocity fields in the Earth's boundary layer is clearly demonstrated. With the help of relatively inexpensive equipment it should now be possible for the meteorologist to gain new insight into such problems as low level heat flux, air-sea interaction and exchange processes, and the structure and motion of wave phenomena. As was stressed earlier⁶, it should also be possible to determine the variation with height and time of the temperature structure constant, C_T (that is, of the r.m.s. difference in atmospheric temperature at two points unit distance apart). This parameter is of importance in the field of optical communications, because it is linearly related to the structure constant C_n , which describes the intensity of small scale fluctuations of optical refractive index.

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Protective Immunity to Malaria provides a Model for the Survival of Cells in an Immunologically Hostile Environment

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Protective immunity includes cooperation between two lymphocyte populations and macrophage activation, but parasites survive by frequent changes of certain antigens. Continued multiplication of other antigenic cells may involve similar antigenic variation.

PARASITIC protozoa, like many bacteria and neoplasms, can survive long after the host has responded to their presence with a vigorous immune reaction. In trypanosomiasis^{1,2}, malaria^{3,4} and probably babesiasis⁵, the parasite evades the host's immune response by repeated changes of certain antigens. A single infective inoculum produces a succession of antigenic variants (several may be present in the blood at one time), and each variant requires the elaboration of a specific immune response for its destruction; different strains have a different repertoire of variants⁶. In malaria, the demonstration of antigenic variation explained the chronicity of erythrocytic infections but left unanswered three important problems concerning the protective response to this stage of the parasite's life cycle.

First, there was the question of strain specific protective immunity. Typically the asexual blood stage of malaria begins with an intense parasitaemia. If the host survives, the number of parasites then decreases to give a sparse although fluctuating blood infection which persists for many months. Because all antigenic variants produced during a chronic infection are fully virulent for non-immune hosts⁷, the development of a protective response was postulated, transcending variation and capable of inhibiting although not entirely preventing multiplication of new variants^{3,4}. The nature of this variant transcending immunity was obscure but largely strain specific⁸.

The second problem was non-specific protection. Slight cross protection develops between strains known to be antigenically distinct and to have a different repertoire of variants^{6,8}. This cross protection appears most effective if the host has a subpatent infection when challenged with a second strain. Finally, there were late recrudescences. Sudden and sometimes fatal recrudescences can occur in chronic infection.

Here I am concerned with two of these problems, the strain specific and non-specific protective responses. The implications of these findings for analogous situations where parasites or neoplastic cells persist in an immunologically competent

host will also be discussed. Details of the experiments will be published elsewhere.

Strain Specific Protective Response

I shall consider first the variant specificity of protective antibody. Passive transfer experiments have shown that in malaria⁹ and trypanosomiasis¹⁰ antibodies are protective, and in African trypanosomiasis protective antibodies are known to be solely variant specific^{11,12}. With malaria, the design of most passive transfer experiments and some *in vitro* tests^{13,6} has taken no account of the parasite's antigenic variability⁹, but the relatively poor protection obtained suggests that the parasites could circumvent the effects of antibody by a change of antigenicity¹⁴. Support for the concept that protective antibody is variant specific in malaria, as in trypanosomiasis, comes from previous work which demonstrated that *in vitro* opsonizing antibodies are variant specific⁶; and that *in vivo* opsonization is a major factor in protection against malaria¹⁵. There is now evidence from a passive transfer experiment of the variant specificity of protective antibodies in serum collected from a monkey after *P. knowlesi* infection.

A monkey was sensitized with an emulsion of dead *P. knowlesi*-infected red cells in Freund's complete adjuvant and subsequently infected⁸. When parasites were detected in blood smears, a sample of blood was frozen as a viable parasite stablate. The monkey was killed and exsanguinated for serum 22 days after infection; a subinoculation test showed that the animal had eradicated its infection. 10⁴ parasites derived from that stablate were used to infect four normal monkeys and 28 h later two were given 7 ml/kg body weight of serum collected from the monkey providing the stablate; the other two animals were given an equal amount of normal serum. The parasitaemia was monitored by daily blood smears. Fig. 1 shows that the serum had some protective effect, but nevertheless the monkey died of a fulminating parasitaemia. Schizont-infected cell agglutination tests³ (Table 1) showed quite clearly that the antigenic character of the population producing the fatal infection in the recipient was different from the original population injected, the one which the donor of the stablate and serum had eliminated. Tests on the serum used in this case and on other sera also suggest that protection correlates with the presence in the serum of opsonizing antibodies.

In the absence of any convincing demonstration of protective antibodies of wider specificity, this evidence, together with earlier experiments with monkeys immunized with a specific variant⁶⁻⁸, indicates that, as in trypanosomiasis, humoral protection in malaria is mediated through a succession of

variant specific antibody responses. Thus by implication the decrease in the level of parasitaemia associated with the chronic phase of the infection is not due to production of protective antibodies reacting with antigens common to all variants but rather to cellular changes associated with the infection and their effect on antibody synthesis and action.

Thymus-derived Cells

Protective immunity to malaria in rodents is thymus dependent¹⁶ and the allergic response by primates to malaria infection also involves thymus-derived cells¹⁷. Specifically sensitized thymus-derived lymphocytes apparently have no cytotoxic effect on red cells containing malaria parasites¹⁸, suggesting that the importance of this lymphocyte population in malaria immunity is related to the production of protective antibodies.

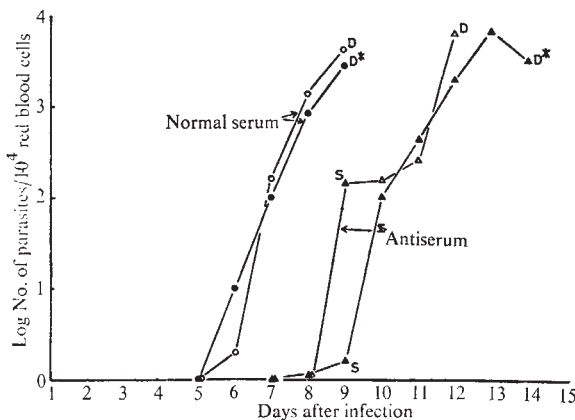


Fig. 1 The protective effect of antiserum of known specificity against the homologous antigenic variant: *P. knowlesi* in *M. mulatta*. S, Parasites subinoculated into a non-immune monkey to provide schizont-infected cells used in agglutination test (Table 1); D, died. Monkeys were infected with 10^4 *P. knowlesi*-infected erythrocytes and given 7 ml./kg body weight of serum intravenously 28 h later.

* These animals were sensitized with Bacille Calmette-Guérin (BCG) intradermally and then given BCG intravenously 15 days before infection.

In *P. knowlesi* infection, specific antibody synthesis to variants appearing at the beginning of the infection is typical of a primary response but antibody production to later variants, when the parasitaemia is low, has the kinetics of a secondary response⁷. This faster response to later variants probably explains their rapid removal from the circulation and the drop in the overall level of parasitaemia. We know that protection produced by these infections is largely strain specific and that strain specific protection is related to the presence of antigenic determinants on the surface of the parasitized red cell which are common to all variants of the strain used; strains with different common determinants show little cross protection^{8,6}.

There is considerable evidence from model systems that, in many cases, an effective antibody response to one antigenic determinant often requires the simultaneous recognition of another distinct but associated determinant¹⁹. Synergistic effects by two separate antigenic sites on isologous red cells²⁰ are of particular interest in this context since the antigenic stimulus provided by malaria parasites has been likened to immunization with foreign red cells⁷. In other experiments using artificial haptens coupled to protein carriers, presensitization with the carrier protein results in an anamnestic hapten specific response to the first injection of the hapten coupled to the carrier. Also after presensitization with a carrier coupled with one hapten, injection of another hapten, coupled to the same carrier, can result in an anamnestic

response specific for the second hapten²¹. Pretreatment with the carrier is thought to result in the production of receptors that can recognize the carrier, and the abundance of these receptors in the secondary lymphoid tissue facilitates synthesis of hapten specific antibody¹⁹.

Table 1 Reciprocal Titre of Schizont Infected Cell Agglutinins to the Population of *P. knowlesi* used to infect Antiserum Recipient and to the Population isolated on Day 9 after Serum Transfer

	Population used to infect	Population isolated from blood on day 9
Normal serum	< 10	< 10
Passive transfer serum	681,250	< 10
Chronic serum *	31,250	31,250

* Serum from monkey with history of 14 months' infection with the same strain of *P. knowlesi*.

By analogy, erythrocytes infected with malaria parasites can be likened to a carrier protein (the strain determinants), having one of an interchangeable series of haptens (variant specific determinants). Early parasitaemias could result in the production of an abundance of receptors to strain determinant(s) which could then facilitate a rapid secondary type response to later variants.

Another aspect of the immune response relevant here is that in mice, at least, the antibody response to foreign red cells requires the sensitization and simultaneous presence of at least two distinct populations of immunocompetent cells, one of which is apparently derived directly from the bone marrow (B lymphocytes), and one derived from the thymus (T lymphocytes)^{22,23}. B lymphocytes apparently differentiate to produce antibody specific for the determinant they recognize, but to do this satisfactorily they need the simultaneous presence of T lymphocytes, which probably recognize different determinants on the antigen and act as "helper" cells for the antibody-producing B population. Evidence suggests this is also often the case with protein carriers and haptens²⁴.

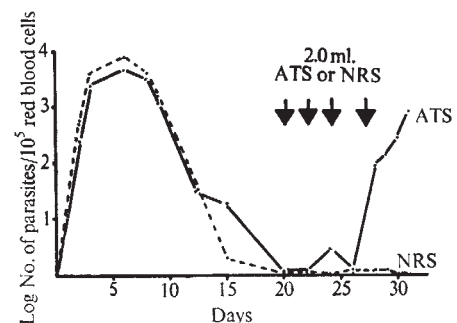


Fig. 2 The effect of ATS (absorbed with rat erythrocytes) on the parasitaemia in rats chronically infected with *P. berghei*. ATS or NRS (2.0 ml.) was injected intraperitoneally on the days indicated. Each point is the geometric mean of results for ten rats.

In malaria, if T lymphocytes recognize the surface determinants common to all variants, and act as "helper" cells in the production of variant specific antibody by cells derived from B lymphocytes, which respond to the variant specific determinant, then a build-up of helper cells in the spleen would result in a more rapid secondary type response to new variants (as in the carrier hapten situation which I have outlined). Thus variants appearing at this stage would be

much more rapidly destroyed and the infection maintained at a low level. Protection, although strain specific, would be mediated through antibodies specific for variants of the strain used. Experiments with rats chronically infected with *P. berghei* support this hypothesis.

Selective Depletion

Anti-thymocyte serum (ATS) selectively depletes lymphoid tissue of thymus-derived lymphocytes²⁵ and reduces the antibody response to several antigens, including foreign red cells, but this effect can be reversed by the injection of thymus cells²⁶. If the hypothesis outlined here is correct, then ATS treatment of a chronically infected animal should induce a recrudescence of the infection by the particular variant in the blood at the time and reduce the ability of lymphoid cells from these animals to transfer the protective immunity of chronic infection (unpublished work of R. S. Phillips) to intact non-immune recipients. A recrudescence might result in part from dysfunction of the reticulo-endothelial system (RES) produced by the ATS^{27,28}, but the reduced protection on adoptive transfer should be independent of this effect because in the cell recipients the RES would be intact.

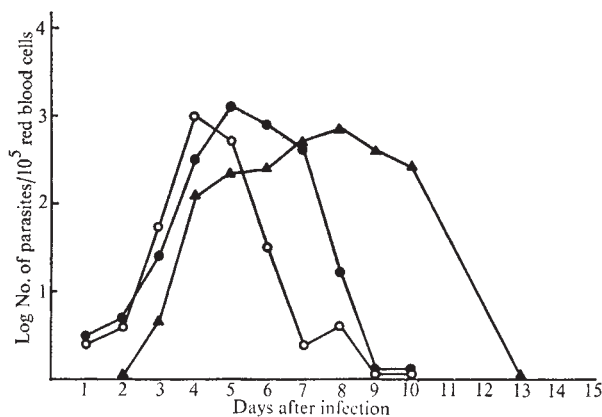


Fig. 3 The parasitaemia in rats receiving 2×10^6 *P. berghei*-infected erythrocytes and spleen and lymph node cells from (●) chronically infected rats treated with ATS; (○) chronically infected rats treated with NRS; and uninfected rats treated with ATS (▲). Each point is the geometrical mean of results for five rats.

Inbred male rats (NIMR August strain, 120–150 g), with a 3 week old infection which had become subpatent, were divided into groups of ten. On days 20, 22, 24 and 27, they were inoculated intraperitoneally with 2.0 ml. of anti-rat (ATS)²⁹ or normal rabbit serum (NRS). The effect on the parasitaemia in these rats is shown in Fig. 2. On day 31 they were exsanguinated for serum, and cell suspensions prepared from their spleens, mesenteric and cervical lymph nodes; these cell suspensions were then transferred into non-immune male recipients at a donor recipient ratio of 1 : 1. In one experiment, the recipients received on day 0 an additional inoculum of 2×10^6 parasites derived from the *P. berghei* stabilate originally used to infect the donor rats. In the other experiment the infection in the recipients was derived solely from the parasites transferred in the spleen cell suspension. The results (Fig. 3) were essentially similar in both experiments. Additional controls included one group which received cells from both ATS and NRS-treated donors and one group which received cells from ATS-treated uninfected donors. The infection in rats receiving a combination of cells from both ATS and NRS-treated donors was identical with

that of the recipients of cells from infected NRS-treated donors alone; and recipients of cells from ATS-treated uninfected donors behave like recipients of cells from intact uninfected donors.

In the experiment where no additional parasite inoculum was given to the recipients, the parasite population developing in the recipients was tested for sensitivity to serum collected from donors at the time of cell transfer in comparison with the original stabilate used to infect the donors. The results are given in Fig. 4.

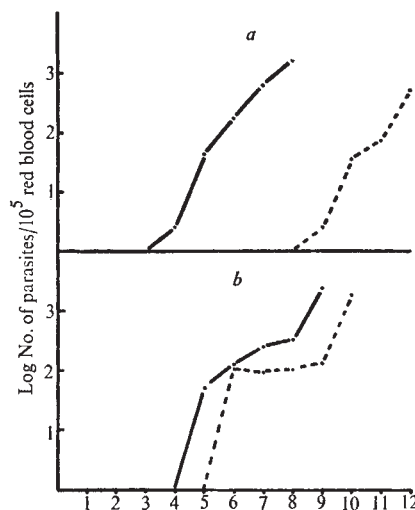


Fig. 4 Parasitaemia in rats infected with 10^3 *P. berghei*-infected erythrocytes and then either treated with 2.0 ml. of pooled serum collected from the ATS-treated rats in Fig. 2, day 31 (--- ● ---), or normal serum (—●—). The parasite population used was either (a) that used to infect the ATS-treated animals, or (b) isolated after ATS treatment. Each point is the geometrical mean of results for four rats.

ATS treatment induced a recrudescence of the infection in the cell donors (Fig. 2) and significantly reduced but did not abolish the capacity of these cells to transfer the protective immunity typical of chronic infections (Fig. 3). The parasite producing the recrudescence was a new antigenic variant (Fig. 4).

Samples of spleen and lymph nodes taken from the cell donors were examined histologically for the effects of ATS and NRS treatment (by courtesy of Mrs E. Simpson of this institute). The thymus-dependent areas of lymphoid tissue were found to be selectively depleted in the ATS-treated donors. In the infected donors, treated with ATS or NRS, there was massive proliferation of plasma cells, probably reflecting the animals' experience of a succession of parasite antigenic variants. In NRS-treated donors there was no loss of thymus-dependent areas and with the uninfected ATS-treated donors there was no unusual proliferation of plasma cells.

This result shows that thymus-derived cells are important in the maintenance of immunity in chronic infections and it is consistent with the hypothesis that their role is that of "helper" cell for variant specific antibody synthesis by B cells. The increased responsiveness to new variants which controls the infection apparently depends on an accumulation of sensitized thymus-derived cells in the lymphoid tissue.

Non-specific Protection

Slight cross protection occurs between strains of *P. knowlesi* differing in their surface antigens^{8,6}. This effect requires explanation, as does the fact that specific protection is more effectively transferred by serum if the recipient is already infected with another species of malaria³⁰.

The histology of the RES system of animals with malaria¹⁵ is similar to that in mice showing macrophage "activation" resulting from *Listeria* infection³¹, and induced by sensitization of thymus-derived lymphocytes³². Because opsonization by specific antibody is important in protective immunity to malaria, we tested whether "activated" macrophages could phagocytose erythrocytes infected with malaria parasites more readily than normal macrophages.

Parkes MD strain mice (22–25 mg) were infected with 0.2 ml. of a suspension of reconstituted Glaxo freeze-dried BCG vaccine. Monolayers of peritoneal cells from uninfected control and BCG-infected mice were collected 28 and 34 days after infection, and compared for their ability to phagocytose red cells infected with *P. knowlesi*⁶ in the presence of a weakly opsonizing antiserum. The number of *P. knowlesi*-infected erythrocytes phagocytosed, and the spreading activity of the macrophages as a measure of their "activation", was determined from photomicrographs of the monolayers stained with Giemsa solution 3 h after collection of the cells and 2 h after adding the parasitized erythrocytes. The results are given in Table 2.

Table 2 Number of *P. knowlesi* Infected Erythrocytes Phagocytosed, and Spreading Activity, by Macrophages from BCG Infected and Normal Mice

	BCG		Normal	
	Opsonizing serum	Normal serum	Opsonizing serum	Normal serum
Parasitized cells/100 macrophages	270	3	36	0
Mean diameter of macrophages (μm)	19.0 (10–32)		10.0 (8–15)	

Macrophages from BCG-infected mice phagocytosed *P. knowlesi*-infected red cells more readily than macrophages from normal mice in the presence of specific antibody; virtually no phagocytosis occurred in the absence of opsonizing antibody. This result and the histological evidence for RES hyperactivity in malaria indicate that the less specific aspects of malaria immunity can be attributed to RES activation (possibly induced by specifically sensitized thymus-derived lymphocytes³²) reinforcing the specific antibody response.

Protective Immunity to Malaria

In typical non-fatal malaria, the protective response apparently has the following characteristics. (1) It is mediated through protective antibodies, opsonins and possible anti-merozoite antibodies, which are solely variant specific. (2) Effective synthesis of these antibodies probably depends on sensitization of thymus-derived lymphocytes by surface antigens common to all antigenic variants; these lymphocytes then act as "helper" cells in antibody production. (3) Accumulation of "helper" cells in the spleen enables the host to respond more quickly to new variants and thus reduce the level of infection. In animals which have reduced the infection to submicroscopic levels, selective depletion of the thymus-derived cells from the spleen and lymph nodes impairs the protective response and allows increased multiplication of the variant present in the blood at the time. (4) Macrophage "activation" enhances the effect of variant specific opsonizing antibodies.

Malaria still remains one of the principal causes of human mortality and morbidity and an important subject in its own right, but it is equally important as a model for other infections. Intra-strain antigenic variation has been described in three genera of parasitic protozoa^{1–5} and analogous

adaptability is known in some free living species³³. In malaria, variation apparently occurs in populations as small as 10³ (and possibly less) and can be induced in animals which have variant specific but non-protective antibodies^{6–8}.

A high frequency of variation occurs in chronic infections (to an extent which is rarely appreciated by workers in other areas of immunology). It is probably a feature of most protozoal infections, although the effector mechanisms by which variants are destroyed will depend on the habitat of the parasite³⁴. In some circumstances variation may lead to an overwhelming parasitaemia. It has also been described in the chronic spirochaete infection, relapsing fever¹², and model *Treponema* infections suggest that it also occurs in yaws³⁵ and syphilis³⁶ (and C. D. Ginger, personal communication).

Antigenic changes are common in other bacteria and many serotypes have been described, but no attempt has been made to look for their sequential appearance in chronic bacterial (for example, *Salmonella*) infections. In metazoa, parasitic nematodes, *Nippostrongylus brasiliensis*, developing in a host sensitized by a previous infection are antigenically different from those developing in a non-immune host³⁷. The adaptation of schistosomes to their hosts may involve similar mechanisms³⁸.

The events leading to malignant growth and the immunological responses which result are undoubtedly complex and some antibodies can apparently protect tumour cells from destruction³⁹. Nevertheless, the paradoxical growth of many neoplasms *in vivo* when *in vitro* autochthonous cells or plasma are apparently cytotoxic for the neoplastic cells⁴⁰, and the high death rate of tumour cells⁴¹ *in vivo*, indicating that the neoplastic population is continuously eroded by a protective immune response, strongly suggest that antigenic variation occurs in these tumours. Neoplasms produced by chemical carcinogens show diverse antigenic specificities⁴², and clones of different antigenic properties have been isolated from individual tumours⁴³. The apparent depression of gene action in neoplasia^{44,45} might well allow for much greater variation than the limited TL antigen modulation detected using iso-antibodies⁴⁶. If antigenic variation occurs in progressively growing tumours, then the antigens concerned will change repeatedly and will be tumour specific. Antigens with these properties will only be detectable using autochthonous or isogenic systems and sequential samples of tumour cells and host lymphoid cells or sera (see ref. 7).

Adequate antigen recognition, "helper" activity and macrophage activation, may be necessary for an effective protective response to many infections and neoplasms^{32,47,48}, but a full explanation for the continued presence and multiplication of bacteria, protozoa and neoplasms may require in addition an awareness of the capacity of both prokaryotic and eukaryotic cells for antigenic variation. This variation is itself a reflexion of the adaptability of cells and cell populations to a dynamic environment and of the lability of cell surface membranes⁴⁹. It may be significant in this context that monkeys normally highly susceptible to the antigenically labile *P. knowlesi*, when sensitized with *P. knowlesi* antigen in a way which induces a marked thymus-derived cell response, develop a protective immune response of most surprising effectiveness^{8,18}.

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Distinguishing between the Chromosomes involved in Down's Syndrome (Trisomy 21) and Chronic Myeloid Leukaemia (Ph^1) by Fluorescence

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Fluorescence with quinacrine dihydrochloride has been used to identify chromosomes of the human G group.

THE morphological identification of human autosomes within certain chromosome groups (*B*, *C*, *D*, *F* and *G*) is particularly difficult and unreliable¹, but the fluorometric work of Caspersson *et al.*^{2,3}, on chromosomes stained with quinacrine mustard and viewed under ultraviolet light, has shown that the autosomes can be distinguished by differences in fluorescence patterns. We have confirmed these findings using quinacrine dihydrochloride⁴ which, although forming a less stable attachment to the chromosomes, nevertheless gives clear banding patterns so that all twenty-two autosomal pairs can be identified without recourse to fluorometry (Fig. 1). The karyotype in Fig. 1 conforms as closely as possible with the generally accepted arrangement¹ and corresponds with the classification suggested by Caspersson³. Within the *C* group the only notable morphological feature following conventional staining is the secondary constriction in chromosome 9 and we have

confirmed the correct allocation of this chromosome by comparative studies on cells stained with quinacrine followed by orcein. The secondary constrictions in chromosomes⁵ other than number 9 also coincide with regions of negative fluorescence, though not all areas of negative fluorescence show secondary constrictions.

The ability to distinguish between autosomes within the *D* and *G* groups has enabled us to identify: (i) the *G* chromosome trisomic in Down's syndrome⁶; (ii) the *D* and *G* chromosomes involved in some *D/G* and *G/G* translocation mongols⁷; and (iii) the deleted chromosome (Ph^1) associated with chronic myeloid leukaemia⁸. In this communication we show that different *G* chromosomes are involved in these two conditions.

Chromosome preparations from peripheral blood⁹ from ten patients with Down's syndrome were studied; two males and two females were 47, *G*+, two males and three females were 46, *D*-, *t*(*DqGq*)+, and one patient was 46, *XX*, *G*-, *t*(*GqGq*)+. Examination of the preparations made from these individuals showed that in all cases the extra *G* group chromosome (number 21) belonged to the shorter pair with the strongly fluorescent band in the proximal region of the long arms. This was apparent both with the regular trisomic conditions (Fig. 1) and where trisomy involved a translocation with either a *D* or

another *G* group chromosome (Fig. 2). One of the *D/G* translocations involved chromosome 13 (Fig. 2a) and the remaining four involved chromosome 14 (Fig. 2b). It should be noted that the *G/G* translocation (Fig. 2c) studied involved

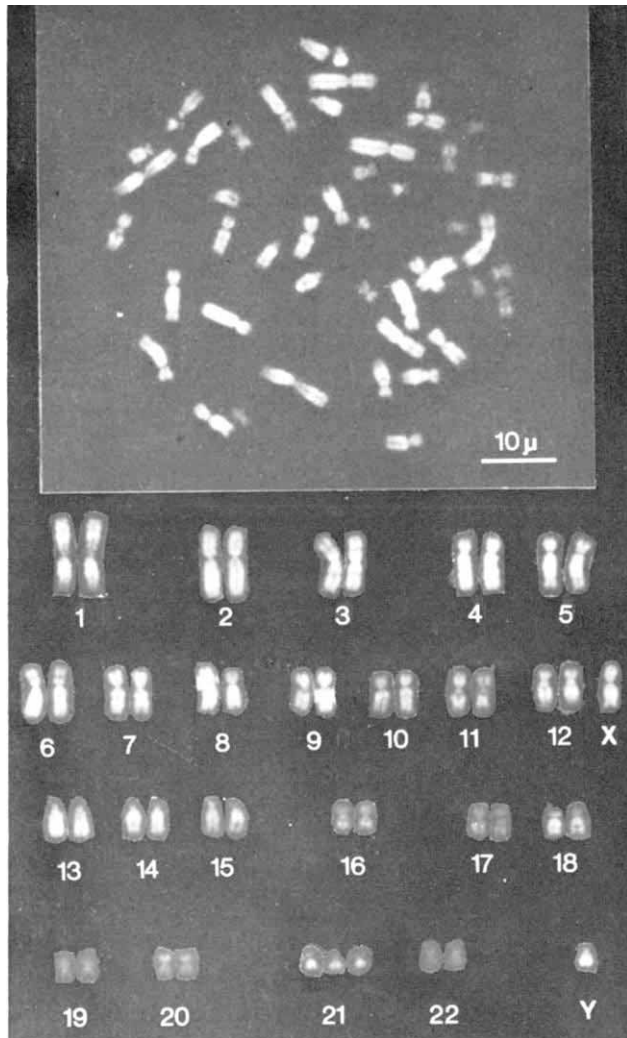


Fig. 1 Metaphase cell with karyotype from a 47, XY, *G*+ individual. All the chromosomes show characteristic fluorescence patterns which enable them to be identified unambiguously.

an exchange between two highly fluorescent chromosomes (21/21) and that if such a state were present in a carrier it would result in all viable offspring being trisomic for the Down's chromosome.

For comparison with the results obtained with Down's syndrome, one male and three female patients who had developed chronic myeloid leukaemia were studied. Three were examined before the start of antileukaemic therapy. Chromosome preparations were made from peripheral blood cultured both with and without the addition of phytohaemagglutinin (PHA).

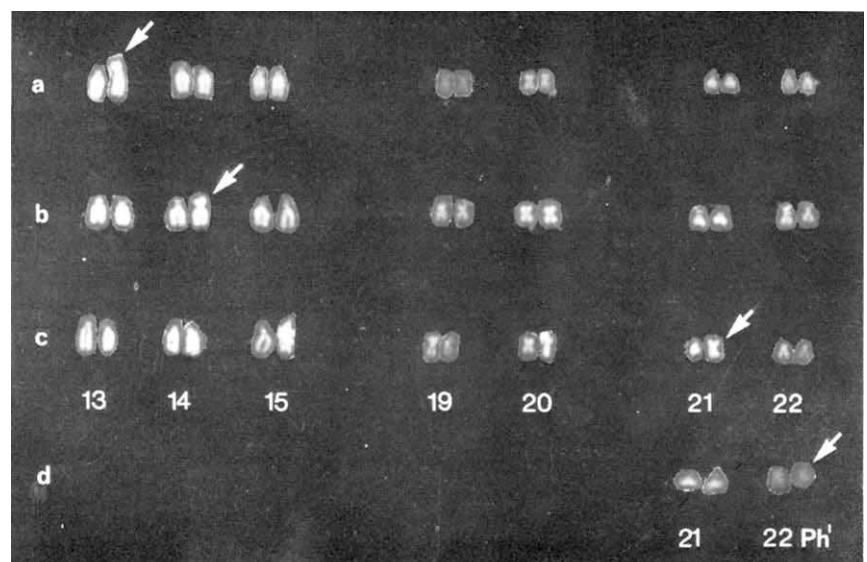
PHA-stimulated leucocytes from these patients had four *G* autosomes with the standard normal fluorescence patterns in cells of normal karyotype. Examination of the few cells which contained the *Ph*¹ chromosome in PHA-stimulated cultures, and the mitotic cells from the unstimulated cultures, showed that in all cases the deleted *G* chromosome was a member of the weakly fluorescent pair (Fig. 2d).

The *G* chromosome that is trisomic in Down's syndrome is conventionally referred to as chromosome 21 and is now shown to be the *G* chromosome with a distinctive fluorescent band. The deleted *G* chromosome (*Ph*¹) in chronic myeloid leukaemia, however, is clearly a member of the weakly fluorescing pair and must therefore be chromosome 22. This result is somewhat unexpected, as the evidence from the elevation of leucocyte alkaline phosphatase activity in cells from Down's patients and the reduced levels found in chronic myeloid leukaemia had strongly suggested¹⁰ that an abnormality involving the same chromosome would be associated with both disorders.

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Fig. 2 Partial karyotypes from: a, 46, XX, 13-, *t*(13q21q)+; note that the fluorescent band on the short arm of chromosome 13 is clearly evident in the translocated chromosome, implying loss of the centromere of chromosome 21; b, 46, XX, 14-, *t*(14q21q)+; c, 46, XX, 21-, *t*(21q21q)+, or 46, XX, 21q-, *t*(21q21q)+; d, 46, XX, 22q- (*Ph*¹).



LETTERS TO NATURE

PHYSICAL SCIENCES

Effect of Apollo 11 Lunar Samples on Terrestrial Microorganisms

No living organisms were found in the Apollo 11 lunar material by the biological quarantine team at the Manned Spacecraft Center, Houston^{1,2}. The same result was obtained in our independent search for living organisms in 50 g of Apollo 11 bulk fines, although iron-containing artefacts superficially resembling microbial clones were seen^{3,4}. Several plausible explanations can be offered for the failure to detect living organisms in this lunar material. First, no living organisms were present in the sample when tested. Second, living organisms were present but the media and environments in which they were placed were not suitable for growth. A third possibility is that living organisms were present in suitable media and environments but that the lunar material inhibited growth. Taylor *et al.*⁵ reported that aqueous extracts of lunar material markedly inhibited the growth and survival of *Pseudomonas aeruginosa*. We now report the results of experiments designed to test Apollo 11 lunar samples for inhibition of the growth of terrestrial microorganisms in conditions resembling as closely as possible those used in our earlier search for living organisms in Apollo 11 lunar material.

The limited quantity of lunar material available to us greatly restricted the number of microorganisms, media and environments that could be tested, but we selected various microorganisms which were capable of confluent growth on the surface of three agar media formulations used in our earlier work^{3,4} as well as several additional media formulations (Table 1).

The assay for inhibition of microbial growth was similar to the well known antibiotic sensitivity disk technique. The surface of agar media in Petri dishes was inoculated uniformly with 0.2 ml. of an 18–24 h liquid culture of the microorganisms (for the green alga *Chlamydomonas reinhardtii*, the inoculum was 0.5 ml. of a 7 day liquid culture) and a scoop of lunar material (6.7 mg of bulk fines sample 10089, 1; 4.9 mg of core sample 10005, 33) was deposited in a clump on the agar surface. In a variation of this technique, lunar material was deposited in a clump on the surface of the agar medium but allowed to incubate for 7 days before inoculation. Controls were agar media, inoculated as described, but without lunar material. Incubation was at 30° C in air in the dark with the exception of *C. reinhardtii* which was incubated in air at 23°–25° C under fluorescent lamps. After incubation the Petri dishes were examined for inhibition of growth in the zone immediately adjacent to the clump of lunar material.

We found no inhibition of the growth of any of the organisms tested by either of the two lunar samples (Table 1); all cultures formed lawns of confluent growth on agar media contiguous with the lunar material, whether or not lunar material and media were incubated for a preliminary 7 days before inoculation. We did observe what seemed to be heavier growth and/or chlorophyll production by *C. reinhardtii* in the region surrounding bulk fines sample 10089, 1, but this observation

could not be repeated. The lunar material affected microbial pigment production by two of the test organisms. *Serratia marcescens*, which does not form the red pigment prodigiosin on media 2L or 2M, produced the pigment on medium 2L in a circumscribed region around bulk fines sample 10089, 1 (Fig. 1a); whereas *Pseudomonas aeruginosa*, which produces the fluorescent pigment fluorescein on all its test media, did not synthesize the pigment in a similar area around core sample 10005, 33 (Fig. 1b). These observations were repro-

Table 1 Effect of Apollo 11 Lunar Samples on the Growth of Terrestrial Microorganisms

Test organism	Medium	Lunar sample	Growth
Bacteria			
<i>Escherichia coli</i> , K-12	2L, 2M	F	+
<i>Serratia marcescens</i> , ATCC 264	2L, 2M	F	+
<i>Arthrobacter crystallopoietes</i> , ATCC 15481	2L, 2M	F	+
<i>Sarcina lutea</i> , ATCC 381	2L, 2M	F	+
<i>Pseudomonas aeruginosa</i> , ATCC 15422	TGE-1, -2, -3, -4, TGY	C	+
<i>Staphylococcus aureus</i> , AD-189	TGE-1, -2, -3, -4, TGY	C	+
<i>Azotobacter vinelandii</i> OP, ATCC 13705	AZO	C	+
Yeast			
<i>Rhodotorula mucilaginosa</i> 2401	2L, 2M	F	+
<i>Hansenula saturnus</i> 2702	2L, 2M	F	+
Green algae			
<i>Chlamydomonas reinhardtii</i> 21 gr	6L	F	+

Medium 2 contained 1.0 g/l. each of glycerol, the sodium salts of lactate, pyruvate, and acetate, methanol, ethanol, K₂HPO₄, KNO₃, and (NH₄)₂SO₄; 0.2 g/l. each of 18 DL-amino-acids, glycine, 4-hydroxy-L-proline, β-alanine; 0.01 g/l. each of seven purines and pyrimidines; trace quantities of sixteen vitamins; pH adjusted to 7.0. Medium 6 contained 1.0 g/l. each of K₂HPO₄, KNO₃, and (NH₄)₂SO₄. These media contained synthetic sea salt mixture (Aquarium Systems Inc., Wickliffe, Ohio) at 0.1% (L) or 3% (M) concentrations (w/v). Medium TGE-1 contained (g/l.) tryptone (Difco, lot 548506), 5.0; glucose.1H₂O (Baker, lot 34483), 1.0; beef extract (BBL, lot 309644), 3.0. Medium TGE-2 was the same as TGE-1 except that trypticase (BBL, lot 308606) was substituted for tryptone. Medium TGE-3 contained (g/l.) tryptone (Difco, lot 548506), 5.0; glucose.1H₂O (Baker, lot 34483), 1.0; yeast extract (BBL, lot 303660), 3.0. Medium TGE-4 was the same as TGE-3 except that trypticase (BBL, lot 308606) was substituted for tryptone. Medium TGY contained (g/l.) trypticase, 5.0; glucose.1H₂O, 1.0; beef extract, 3.0; yeast extract, 5.0 (supplies and lots as before). Medium AZO contained (g/l.) KH₂PO₄, 0.2; K₂HPO₄, 0.8; MgSO₄. 7H₂O, 0.2; CaSO₄.2H₂O, 0.13; FeCl₃, trace; NaMoO₄, trace; yeast extract (BBL, lot 303660), 0.5; sucrose (Baker, lot 30146), 20.0. All media were solidified as required with 2% (w/v) Difco purified agar, lot 486392. Agar media (30 ml.) were dispensed in 15 × 100 mm plastic Petri dishes. F, Lunar bulk fines sample 10089, 1 (6.7 mg/Petri dish); C, lunar core sample 10005, 33 (4.9 mg/Petri dish); +, growth; -, no growth.

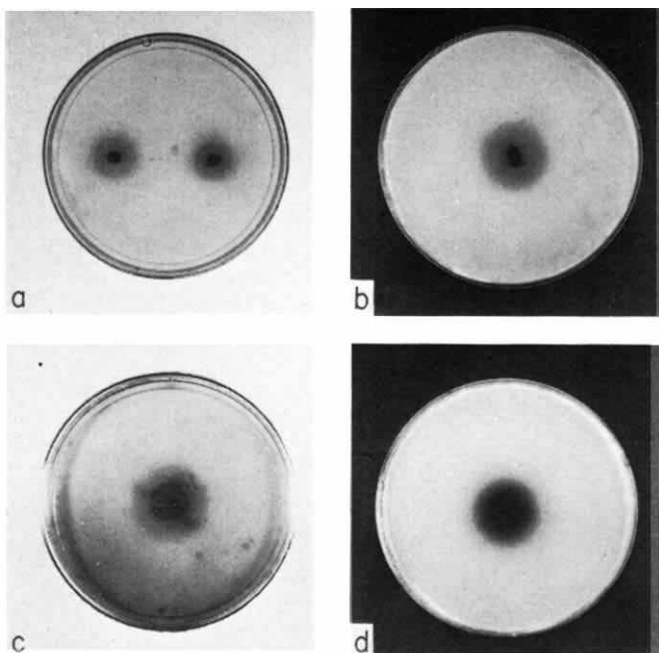


Fig. 1 Effect of lunar samples and iron solutions on microbial pigment production. *a*, *Serratia marcescens*, 24 h Petri dish culture on medium 2L. Pigment production was stimulated in the areas (dark grey) around two clumps (black) of Apollo 11 bulk fines sample 10089, 1. *b*, *Pseudomonas aeruginosa*, 24 h culture on medium TGE-1. Fluorescent pigment production was inhibited in the area (dark grey) around Apollo 11 core sample 10005, 33 (black). These cultures were photographed by fluorescent light emitted under 400 nm ultraviolet excitation. *c*, *S. marcescens*, 24 h culture on medium 2L. Pigment production was stimulated in the area of FeSO_4 addition ($100 \mu\text{g Fe}^{\text{II}}$). *d*, *P. aeruginosa*, 24 h culture on medium TGE-1. Fluorescent pigment production was inhibited in the area of FeSO_4 addition ($500 \mu\text{g Fe}^{\text{II}}$). Photography was as in *b*.

ducible, and suggested that pigment formation was being influenced by one or more water soluble components of the lunar material diffusing into the agar media.

To identify this component, a number of elements were selected for further testing from among the more abundant macro and trace elements reported in analyses of the Apollo 11 bulk fines⁶⁻¹¹. Solutions of these elements were tested individually for their effects on pigment formation by placing 0.01 ml. or 0.05 ml. droplets on the surface of test media, allowing the liquid to absorb into the agar, and inoculating with the microorganisms either immediately or after a preliminary 7 days incubation. Table 2 shows that only iron (ferrous or ferric) affected pigment production in the same manner as the lunar samples, that is, growth of *S. marcescens*

Table 2 Effect of Some Chemical Elements known to be in Apollo 11 Lunar Material on Microbial Pigment Formation

Element	Concentration ($\mu\text{g}/\text{droplet}$)	Effect	
		<i>S. marcescens</i>	<i>Ps. aeruginosa</i>
Al	100	0	ND
Ca	100	0	ND
Cr	100	0	ND
* Fe^{II}	100	S	I
* Fe^{III}	100	S	I
K	100	0	ND
*Mg	100	0	0
*Mn	100	0	0
Na	100	0	ND
Si	0.4	0	ND
Ti	6	0	ND

* $500 \mu\text{g}/\text{droplet}$ were used with *Ps. aeruginosa*. Media used were 2L for *S. marcescens* and TGE-1, TGE-2, TGE-3, TGE-4 for *Ps. aeruginosa*. 0, No effect; S, stimulatory; I, inhibitory; ND, not determined.

and stimulation of prodigiosin production in the area of iron addition (Fig. 1c); growth of *Ps. aeruginosa* but inhibition of fluorescent pigment production where iron was added (Fig. 1d).

S. marcescens requires iron for prodigiosin synthesis¹²⁻¹⁴, whereas fluorescein production by *Ps. aeruginosa* is inversely related to the amount of iron in the medium¹⁵⁻¹⁷. As reported previously^{3,4}, iron was readily extracted from Apollo 11 bulk fines sample 10089, 1 by mineral acid or neutral solutions and was a principal component of one type of artefact which formed around lunar particles resting on the surface of agar media. Although the lack of lunar material prevented direct analyses from being made for the quantity of iron diffusing into the agar from the lunar samples, some estimate of a possible lower limit can be gained from the fact that *S. marcescens* produced prodigiosin in the absence of lunar material on medium 2L supplemented with as little as $0.05 \mu\text{g}$ of iron/ml. These facts are consistent with and support our conclusion that iron probably was leached from the Apollo 11 bulk fines and core samples in concentrations that affected pigment production in *S. marcescens* and *Ps. aeruginosa* but without discernible adverse effect on the growth of these or any of the other terrestrial microorganisms tested.

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Search for Anti-matter in Primary Cosmic Rays

Cosmic rays provide the only sample of the material outside the solar system which is available for examination and it even seems likely that some part of the cosmic ray flux originates outside our own galaxy. In the circumstances, it is understand-

able that there have been several searches for anti-nuclei in primary cosmic rays, partly so as to throw light on the question of whether the universe is symmetrical in matter and anti-matter. An upper limit of $p^-/p^+ \leq 3 \times 10^{-4}$ has been established¹ for energy $E \leq 200$ MeV while an upper limit on the fraction of anti-nuclei to nuclei with charge $Z \geq 2$ and energies less than 700 MeV/nucleon has been put^{2,3} at 0.6%. All these measurements have been restricted in their energy range by the need to observe particles stopping in nuclear emulsions. Even if matter and anti-matter co-exist on the scale of the universe, it is nevertheless likely that the two forms are segregated on a galactic scale, so that the fraction of particles coming from outside the galaxy is likely to be greater at greater energy. It follows that the higher the energy at which the search is conducted, the more significant will be any upper limit that can be established for the amount of anti-matter. We report a new method which we have used to search for anti-nuclei of charge $Z \geq 6$ in the energy range $5.0 \lesssim E \lesssim 9.0$ GeV/nucleon.

Our method depends on the fact that the geomagnetic threshold rigidity R and therefore the velocity of cosmic rays arriving from the east is lower for negatively charged primaries than for positive primaries. This difference is greatest in the equatorial zone. The detector (Fig. 1) consists of a gas Čerenkov detector combined with two scintillator elements to form a cosmic ray telescope which can be carried by a balloon. At altitude the telescope is suspended with its axis at 50° to the vertical. The gas Čerenkov counter gives a measure of particle velocity and this, together with the computed threshold rigidity, allows the sign of the charge to be determined in the eastern and western quadrants.

The top and bottom elements of the telescope, D_1 and D_3 , are disks of NE 102A plastic scintillator 50 cm in diameter and 0.3 cm thick. These are viewed by three photomultiplier tubes 1A, 1B and three connected in triple coincidence. Together they form a telescope with a half-opening angle of 20° and a geometrical factor of 200 cm² steradian. The high reflectance paint and almost spherical shape of D_1 result in nearly uniform light collection and good charge resolution up to charge $Z=10$. The outputs of the three photomultipliers are each fed into pulse height analysers with 127 channels. Comparison of the three outputs makes it possible to identify and reject events in which particles interact during passage through the telescope.

The gas Čerenkov detector is filled with propylene gas at a pressure of 4.8 atmospheres which gives a well defined velocity

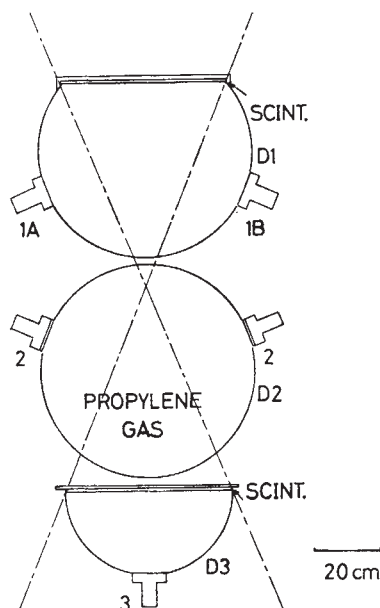


Fig. 1 Schematic diagram of the detector comprising two slabs of plastic scintillator and a gas Čerenkov counter.

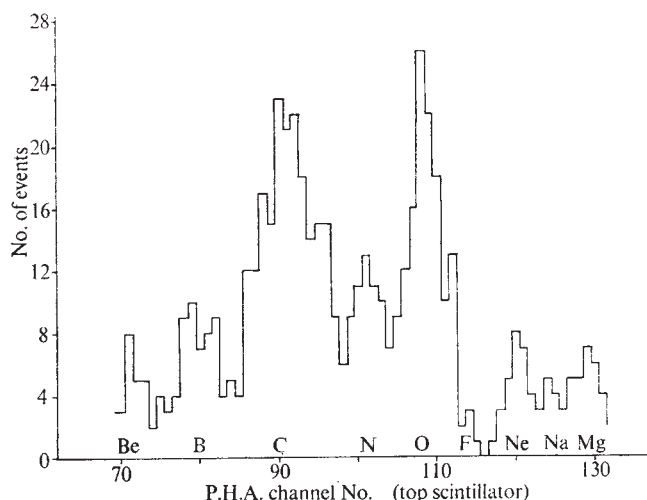


Fig. 2 Pulse height/frequency distribution showing the charge spectrum in the range $4 \leq Z \leq 12$ for cosmic ray primaries with magnetic rigidity greater than 10 GV.

threshold for nuclei with mass to charge ratio $A/Z=2$, corresponding to a magnetic rigidity $R_C=18$ GV. The Čerenkov radiation is detected by two 3.5 inch photomultiplier tubes connected in parallel. Because of the expected large range of outputs a gain-switching circuit giving a dynamic range of 1,600:1 is incorporated in the channel 2 amplifier. The maximum gain was adjusted to allow detection of a single electron released at one of the D_2 photocathodes. A highly relativistic neon nucleus ($Z=10$) gives rise, on average, to the release of 250 photoelectrons. The D_2 output is gated by the telescope coincidence pulses and feeds into a 127 channel pulse height analyser. Data from all channels are fed into stores and sequentially telemetered to the receiving station, as are atmospheric pressure and the temperature of the apparatus, together with the output of a Sun sensor and of a Hall-Probe magnetometer mounted horizontally in the detector. The outputs of these last two devices are used to determine the orientation in azimuth of the detector at any instant during the flight.

Two of these telescopes were built and flown from Kampala (15° N 32° E) on 450,000 foot³ balloons. The telescopes suspended at a zenith angle, $\theta_z=50^\circ$, were free to rotate in azimuth, θ_A . On the first flight the balloon floated at 5 mbar for 8 h and on the second flight at 5 mbar for 4 h. Determination of the geomagnetic cut-off rigidity R_G^+ for positive particles using the trajectory calculation of McCracken *et al.*⁴ shows that the mean value of R_G^+ for $\theta_z=50^\circ$ varies between 10.5 GV for the westerly direction $\theta_A=270^\circ$, and 28 GV for the easterly direction $\theta_A=90^\circ$. Because of the finite opening angle of the telescope, the lowest value of R_G^+ for the easterly direction is 21 GV. In over 100 trajectory calculations no evidence was found for the existence of a penumbral region in the primary cut-off.

The gas Čerenkov threshold corresponds to a magnetic rigidity of 18 GV. Consequently, when the detector is orientated with its axis directed between south-east and north-east ($45^\circ < \theta_A < 135^\circ$) no positive primary with $A/Z \leq 2$ can pass through it without giving an output in the gas Čerenkov channel 2. The minimum number of electrons released at the D_2 photocathodes by a $Z=+3$ primary arriving with $45^\circ < \theta_A < 135^\circ$ is about 15. The possibility of not observing an output from D_2 in these conditions is negligible and any $Z \geq 3$, $A/Z \leq 2$ primary not giving an output must therefore be negatively charged and have an energy between 5.0 and 9.0 GeV/nucleon.

A similar technique has been used by Daniel and Stephens⁵ to deduce the electron-positron ratio in the primary cosmic radiation. In their experiment re-entrant albedo electrons

presented a problem, but this does not arise in the case of relativistic particles with $Z \geq 6$, when the albedo is negligible. Slow protons and slow alpha particles can be identified and eliminated by comparing the energy loss in the top and bottom scintillators.

In Fig. 2 the pulse height/frequency distribution is plotted for the top scintillator. The charge groups from lithium to magnesium can be readily distinguished independently of the initial calibration (carried out using relativistic muons at ground level). By using the ground level calibration, charge values can be assigned for nuclei up to those of iron which form an identifiable group in the charge distribution.

After assigning a charge value to each event, the number of the event was correlated with the orientation of the detector and the Čerenkov channel output. Any particle with $Z \geq 6$ recorded while the telescope was pointing within 60° of east and having zero Čerenkov pulse should be negatively charged. No events of this nature were found and this result sets an upper limit to the fraction of anti-nuclei in the cosmic radiation in the rigidity range 10–18 GV. The number of ordinary nuclei within the same rigidity range is given by the total number of events recorded which had a zero Čerenkov output while pointing due west ($\pm 60^\circ$). The detailed breakdown of the nuclei seen is shown in Fig. 3, where N_H represents the number of heavy nuclei ($Z > 8$). Thirty-eight nuclei were observed which satisfied this criterion.

A small correction is necessary because the time the telescope pointed within 60° of east was 3% greater than the corresponding time for the westerly direction. Thus no anti-nuclei were observed in the time when forty ordinary nuclei would have been observed in the same rigidity range. The ratio of anti-nuclei to nuclei ($Z \geq 6$) in the rigidity range $10 \leq R \leq 18$ GV was therefore

$$N_{\bar{A}}/N_A < 7.5\% \text{ (95\% confidence limit)}$$

The significance of this upper limit can, of course, only be assessed when there is a better understanding of the propagation of cosmic rays in the galaxy. In particular, we need to know what fraction, if any, of the cosmic rays of this rigidity come from outside our own galaxy. At the present time, all we can be sure about in this respect is that the higher the energy of the particle, the greater the probability that they originate in either metagalactic space or in other galaxies. For this reason it is important to carry out the sampling at the highest possible energies.

An indirect and rather uncertain estimate of the upper limit to the number of anti-protons in the primary cosmic ray beam based on observations at ground level has been made by Brooke and Wolfendale⁶ for energies of $\approx 10^{12}$ eV. This result

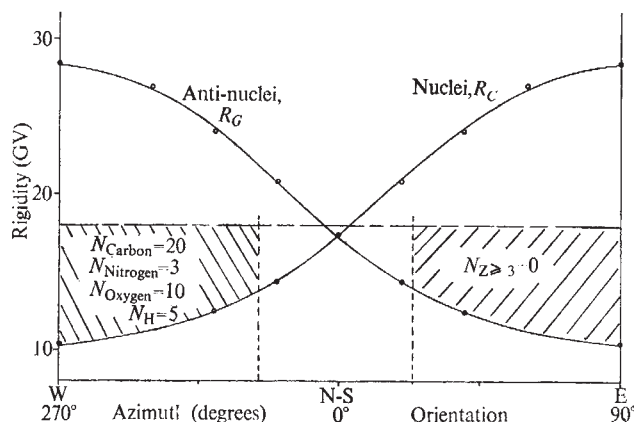


Fig. 3 Azimuthal dependence of the geomagnetic threshold rigidity for positively and negatively charged cosmic ray primaries at the equator. R_C is the minimum rigidity at which the Čerenkov counter registers a signal. The shaded areas are those in which the detector distinguishes unambiguously between positive and negative primaries.

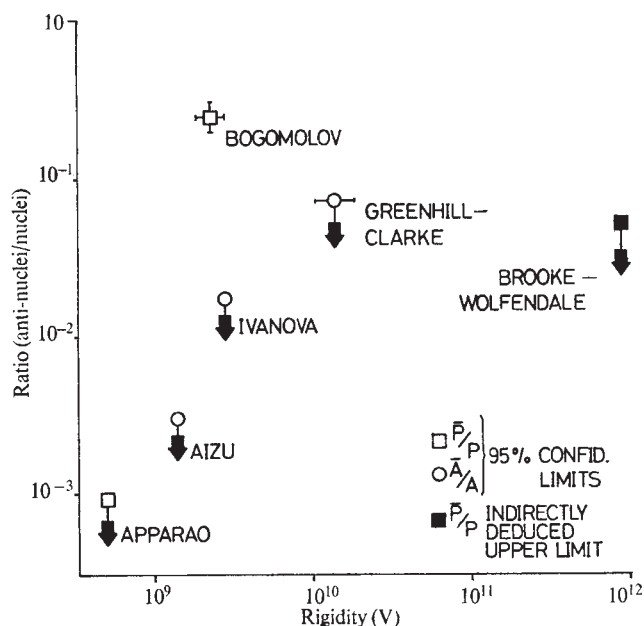


Fig. 4 Summary of present information on fluxes of anti-particles in the primary cosmic rays at 95% confidence limits.

together with the directly established experimental upper limits at the present time are shown in summary in Fig. 4 (refs. 7 and 8).

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Diffuse Interstellar Features in the Spectrum of the Most Heavily Reddened Star

THE Cygnus OB2 stellar association contains the most heavily reddened early type star known^{1,2}. If all interstellar grains were to be removed from the line of sight to this eleventh magnitude star, it would rival Sirius as the brightest in the sky, its B-V colour excess being 3.7 magnitudes³.

A photographic spectrum of the star, at 200 pm (=2 Å) resolution, has been obtained with the Cassegrain spectrograph of the Isaac Newton telescope. The dispersion used

was 6 nm mm^{-1} ; and the wavelength range, 570–675 nm. The spectrum is compared here with that of star number 10 (BD +41° 3804) in the same association, for which two spectra were obtained (with the same instrument).

Microdensitometer traces of spectra of Cygnus OB2 No. 10 and No. 12 are reproduced in Figs. 1 and 2 respectively. Relative intensity scales, marked in percentages, have been inserted at representative points along the traces.

These spectra show the second strongest diffuse interstellar features in the red so far recorded, only the broad feature near 618 nm seen in supernova spectra⁴ being stronger.

The computed equivalent widths of the interstellar features, of H α and of the stellar He I lines, together with central intensities and half-widths for the diffuse features, are presented in Table 1. All measurements of the "6284" line have been corrected for the blend with the terrestrial O₂ "a band", and those of H α in No. 12 for the blend with the stellar C II lines at 657.8 and 658.3 nm. Also, in calculating the half-widths of the diffuse features, I have allowed for instrumental broadening, by applying a first order correction, assuming gaussian profiles. The relative inaccuracy of the measurements for No. 12 shortward of 600 nm stems from the low density of the spectrum in this region: it is a consequence of the extreme reddening of this star that only one short section of its visible spectrum can be correctly exposed at a time.

is linear over this short wavelength range. Relative to this continuous curve, then, Fig. 3a shows the profile of "6180". An extensive apparent emission wing to the violet is indicated.

Fig. 3b shows that both the strength and the profile of "6180" are the same for the two stars Cygnus OB2 No. 10 and No. 12.

Table 1 shows that all diffuse interstellar features are of almost equal strength for the two stars. Yet No. 12 is twice as reddened as No. 10. This discrepancy is remarkable, considering that the stars are in the same association, and that most of the dust responsible for the reddening lies within the association³. It is strong evidence that the diffuse line absorbers cannot be closely associated equally with the various species in the dust population. On the other hand, a common source for all the diffuse features is suggested by the common ratio of intensities for the two stars. (Wampler⁷ has found a similar but less pronounced discrepancy in the central depths of "4430", compared with the reddening, for Cygnus OB2 stars.) Because there exist normal linear relationships between strengths of the diffuse features and interstellar reddening, on a galactic scale (and the correlations are seldom very striking⁷⁻¹⁰) it seems that the features are deficient in No. 12 rather than overabundant in No. 10.

The "mean wavelengths" of the unidentified diffuse lines, quoted in Table 1, are those measured relative to the stellar

Table 1 Spectral Features of the Heavily Reddened Star in Cygnus OB2

Name	Mean wavelength (nm)	Identification	Equivalent width		Central depth		Mean half-width (pm)
			Cyg. OB2 No. 10 (pm)	No. 12 (pm)	Cyg. OB2 No. 10 (%)	No. 12 (%)	
5,780	$\{ 577.6 \pm 0.2 \}$	Unidentified, interstellar	$\{ 100: \}$ 269 $\pm$ 5	280 $\pm$ 30	$\{ 6 \}$ 41	30:	$\{ 1,600 \pm 300 \}$
	$\{ 578.08 \pm 0.06 \}$				$\{ 36 \}$		$\{ 360 \pm 50 \}$
5,797	579.66 ± 0.08	" "	58 $\pm$ 3	50 $\pm$ 20	14	10:	300 $\pm$ 50
6,180	617.6 ± 0.2	" "	360:	$\{ 370: \}$ 432 $\pm$ 15	$\{ 12 \}$	11	2,500 $\pm$ 300
6,203	620.35 ± 0.03	" "	60:	60:	$\{ 14 \}$	13	290 $\pm$ 50
6,270	626.90 ± 0.03	" "	35 $\pm$ 3	37 $\pm$ 5	9	7	280 $\pm$ 50
6,284	628.33 ± 0.03	" "	272 $\pm$ 10	278 $\pm$ 15	45	48	370 $\pm$ 50
6,614	661.35 ± 0.04	" "	29 $\pm$ 2	25 $\pm$ 3	12	10	≤ 200
Wavelength							
D ₁	589.59	Na I, interstellar	85 $\pm$ 10	50 $\pm$ 20			
D ₂	589.00	" "	76 $\pm$ 8	70 $\pm$ 20			
H α	656.28	H I, stellar + circumstellar?	+16 $\pm$ 5	-503 $\pm$ 20	0:	-60	
D ₃	587.56 (blend)	He I, stellar	140 $\pm$ 3	40 $\pm$ 20			
—	667.82	" "	91 $\pm$ 3	76 $\pm$ 3			

An unambiguous determination of the effective stellar continuum position was possible for all lines except "5780" and "6180". The former seems to be superimposed upon a broader feature centred on 577.6 nm (see especially Fig. 1) and I have therefore interpolated the continuum over the interval $\lambda\lambda 576$ –581 nm.

The "6180" absorption resembles the broad features in the blue at $\lambda\lambda 443$ and $\lambda 489$ nm. The errors of the measurements of "6180" quoted in Table 1 are chiefly due to the uncertain interpolation of the stellar continuum: the results have been calculated assuming continuum is regained at $\lambda 614$ and $\lambda 622$ nm. But there is evidence^{5,6} from automated spectrophotometry of image-tube spectra that "4430" has an apparent emission wing to the violet of the main absorption.

As a test for possible extensive wings in "6180", I have determined the ratio of the spectral energy distribution in pairs of stars (a) Cyg. OB2 No. 10 and κ Cas., and (b) Cyg. OB2 No. 12 and No. 10. In each case, the spectra were taken on the same night and processed together. The results are shown in Fig. 3, where magnitude differences (normalized) are plotted against wavenumber. Only the continuum and the "6180" feature were considered. "6180" is virtually cancelled out for pair (b), leaving only the continuous reddening curve, which

He I lines, and averaged for the two stars. The positions of four of the diffuse lines (those at $\lambda\lambda 620.3$, 626.9 , 628.3 , 661.4) could be measured accurately for both stars: all four have wavelengths slightly less in No. 12 than in No. 10, with a mean difference of 80 ± 50 pm (or $30 \pm 20 \text{ km s}^{-1}$). This could be an indication of either real differences in wavelength, or else of a relative radial velocity: if the latter is true, the line-carriers are moving away from No. 12 faster than from No. 10—or, alternatively, towards No. 10 faster than towards No. 12. The rest wavelengths of the lines are not known, so we cannot yet decide which of these alternatives is true.

Two further results are interesting in this connexion. The interstellar sodium D lines in No. 10 have a velocity relative to the He I stellar lines of $0 \pm 20 \text{ km s}^{-1}$. This means that there is no significant mean difference in wavelength for the six diffuse lines in No. 10 (relative now to the D lines) from Merrill and Wilson's⁸ mean values. Also, the core of the strong H α emission in No. 12 has a velocity of $+65 \pm 15 \text{ km s}^{-1}$ relative to He I $\lambda 667.8$ nm. This result suggests there is an infall of matter into this very young star but, without comprehensive measures of the star's radial velocity, such an infall can only be specified as relative to the effective He I absorption region in the star.

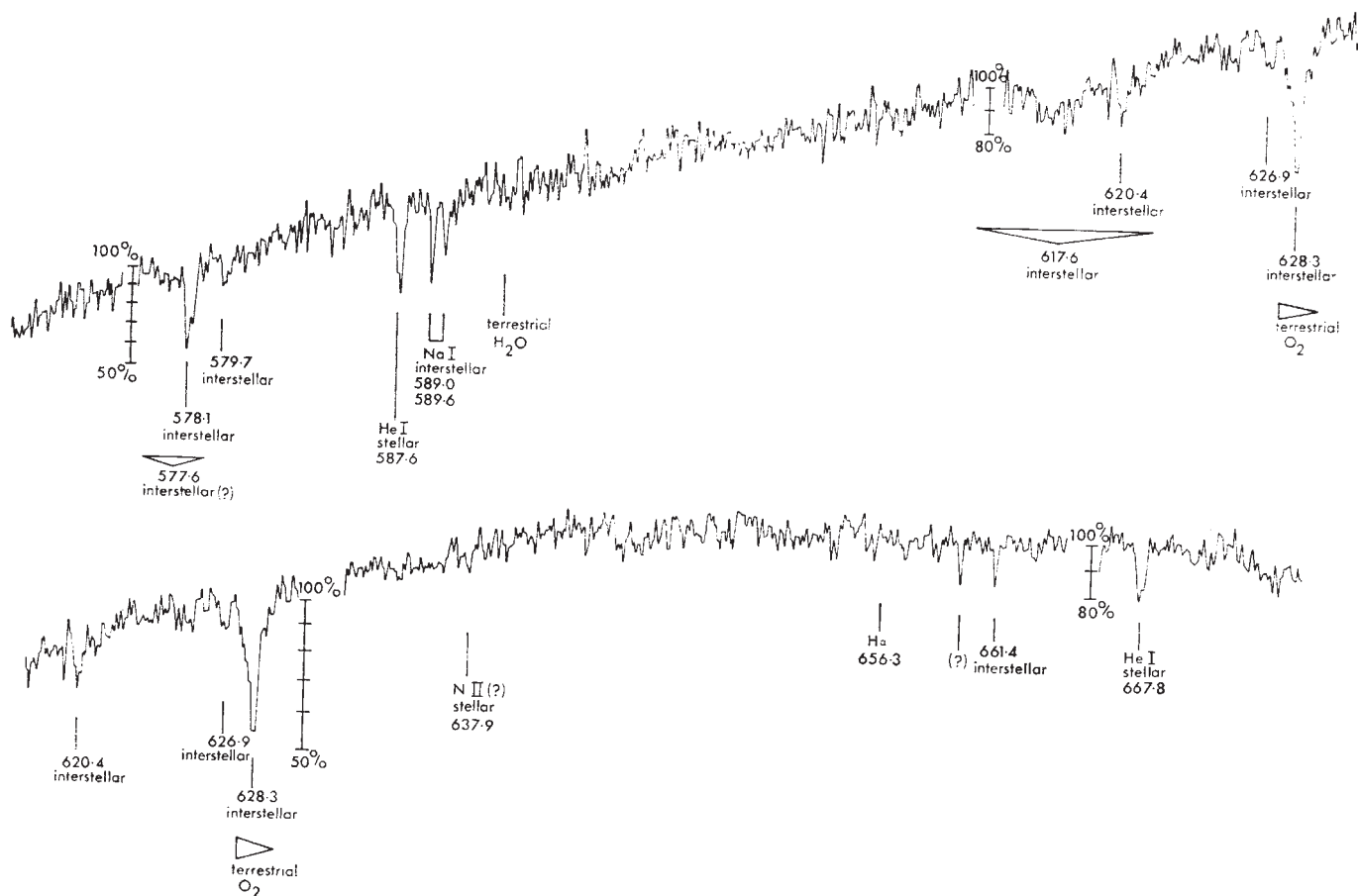


Fig. 1 Microdensitometer traces of spectrum of Cygnus OB2 No. 10: upper trace, $\lambda\lambda 570-631$ nm; lower trace, $\lambda\lambda 618-675$ nm.

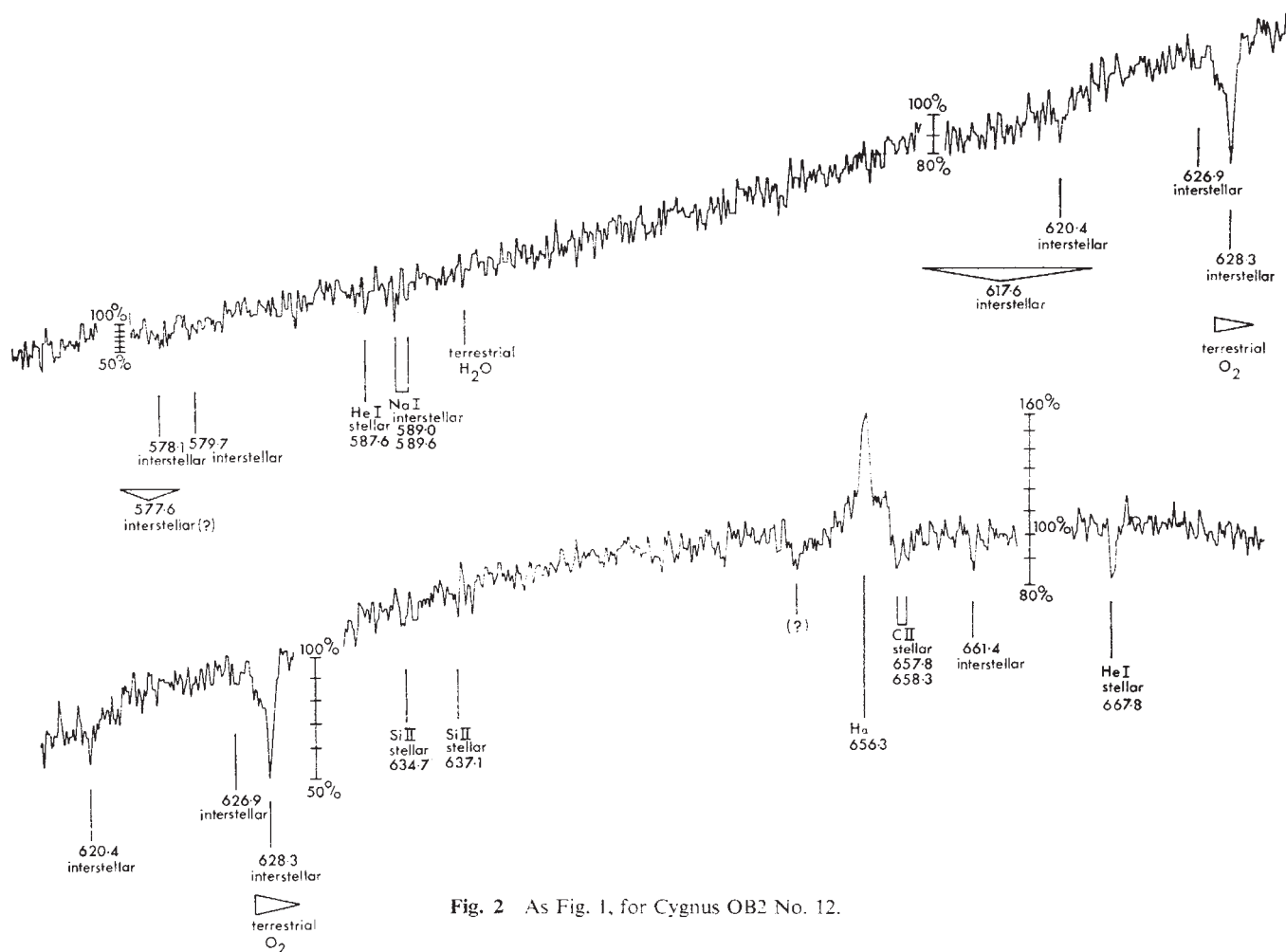


Fig. 2 As Fig. 1, for Cygnus OB2 No. 12.

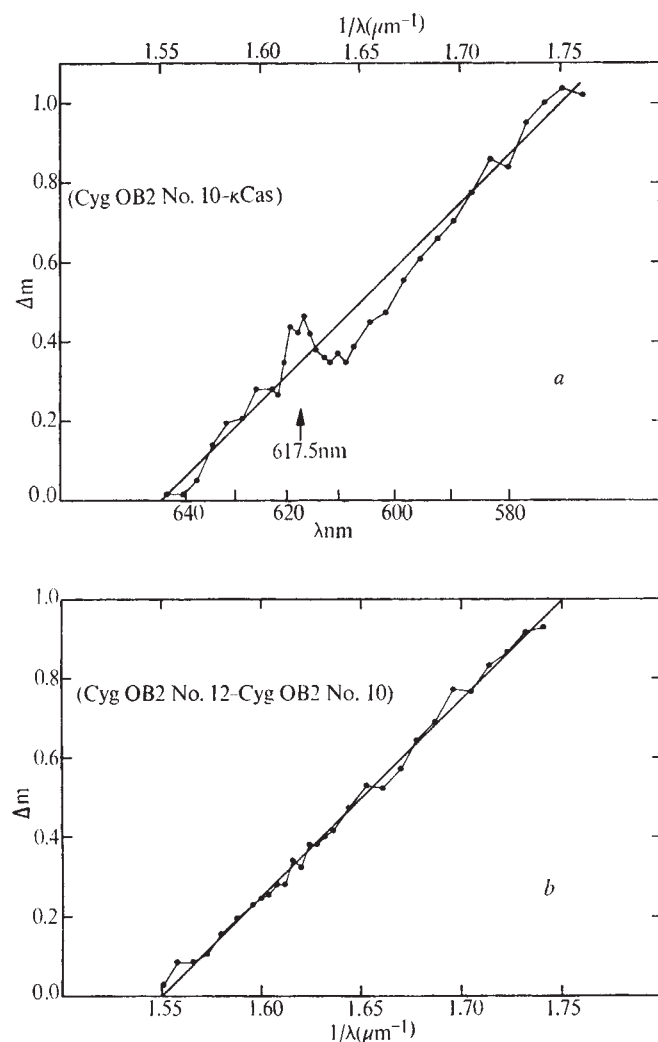


Fig. 3 The reddening curve in the region of the "6180" feature, for Cygnus OB2 No. 10 and No. 12.

The $H\alpha$ emission seems to be closely stellar in origin. The spectrum of No. 12 was obtained using an extended object slit, and the star was not "trailed". $H\alpha$ emission does not extend significantly beyond the stellar image. Taking the distance to the star as 2.1 kpc (ref. 3), the projected seeing and guiding disk is 0.06 pc in diameter and the slit length 0.5 pc. I conclude that there is no detectable circumstellar $H\alpha$ emission between 0.03 and 0.25 pc from Cygnus OB2 No. 12, whereas a halo extending to at least 1 pc is expected for a mid-B type star such as this. The threshold of detection was about 3% of the stellar continuum intensity.

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Elevation of Mid-ocean Ridges and the Evolution of the South-west Indian Ridge

THE distinctive pattern of linear magnetic anomalies parallel to the crests of active spreading centres has been correlated with reversals in the Earth's magnetic field¹ in a manner now universally accepted and used to predict the age of the ocean floor away from the crest of active ridges². The original age correlation, which was made on the assumption of constant spreading on the mid-Atlantic ridge at 20° S, has been strikingly confirmed by the age of the sediments recovered at the base of JOIDES deep sea sediment cores taken along the same profile³.

In many regions of the ocean floor, the magnetic anomaly patterns are not nearly as distinctive. Thus, north-south trending ridges near the magnetic equator will produce magnetic anomaly patterns of low amplitude which are indistinguishable from magnetic noise. Jumps in the position of a spreading centre or changes in its direction will also produce irregular anomalies. Topographic relief on slow spreading ridges (<1 cm/yr) is extreme and the magnetic anomaly patterns across these ridges are also sometimes confused and difficult to interpret.

It may nevertheless still be possible to predict the age of the ocean floor in regions like these. Sediment recovered by the JOIDES deep sea drilling programme³⁻⁵ and magnetic anomaly patterns clearly indicate that the age of the oceanic crust increases with distance away from the crest of the active mid-ocean ridges and Menard⁶ has shown that, for all oceans, the depth of the ocean floor increases with distance from the mid-ocean ridges. There is therefore a rough correlation between depth of crust and age. To investigate this correlation in greater detail, Sclater and Bell⁷ selected topographic profiles in regions of recognizable linear magnetic anomaly patterns in the North Pacific, South Atlantic and Indian Oceans. The depths for a given magnetic anomaly pattern were averaged for each ocean and plotted against age. There is a remarkable correlation between average depth in all three oceans and the age of the oceanic crust (Fig. 1).

In the plate tectonic model of the oceanic crust, the lithosphere is considered at a rigid plate and new material is created at the crest of ridges by intrusion⁸. As this material moves away from the crest it cools and solidifies. Many authors have shown that such a model adequately accounts for the distribution of oceanic heat-flow values⁸⁻¹¹, while the absence of a significant free air gravity anomaly over the ridge crest¹² is

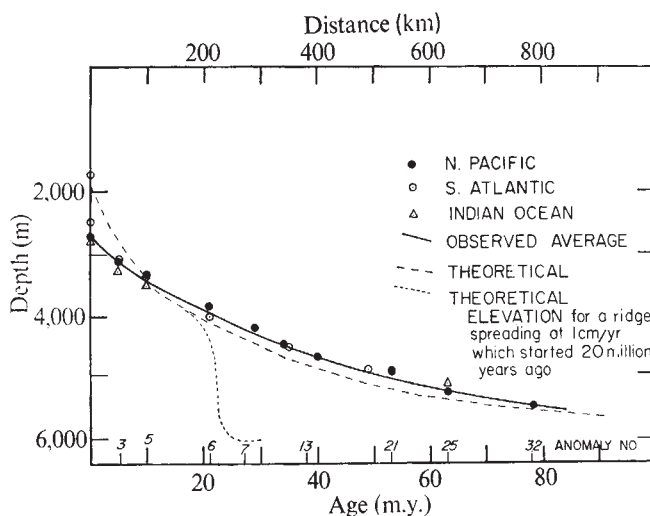


Fig. 1 Plot of elevation of ridge versus age for the North Pacific, South Atlantic and Indian Oceans. The theoretical profile is for a 100 km thick lithosphere with a basal temperature of 1,475° C. The light dashed line represents the theoretical elevation after 20 m.y. for a ridge spreading at 1 cm/yr.

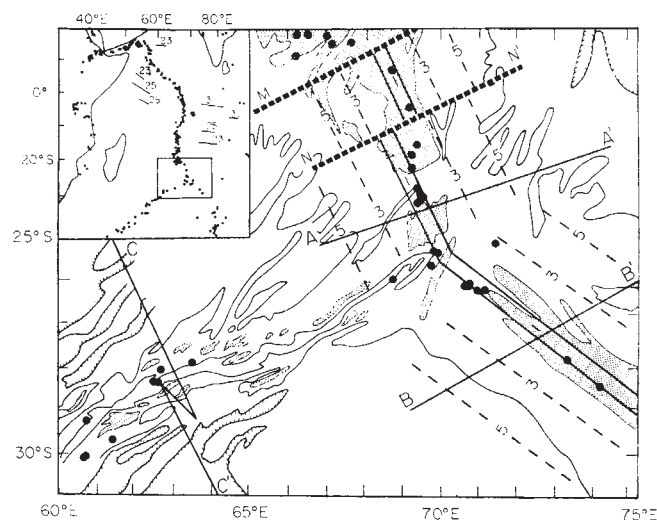


Fig. 2 Magnetic anomaly patterns, fracture zones, earthquakes and elevation for the junction of the central Indian ridge and the south-west and south-east branches. The stippled area represents topography shallower than 3,000 m. The continuous black line is the 4,000 m contour. The hatched line is the 5,000 m contour. The dashed lines represent recognizable magnetic anomaly patterns. The black dots represent earthquakes both on the main diagram and the inset. The numbered lines on the inset mark the position of the recognizable older magnetic anomalies in the western Indian Ocean¹⁵.

evidence that the crust and upper mantle of the Earth must be isostatically compensated. If the base of the lithosphere is taken as the level of compensation¹³, the thermal contraction of a lithosphere 100 km thick with a basal temperature of 1,475° C gives an excellent match to the observed elevation of ridges¹¹ (Fig. 1). The empirical profile is compatible with plate theory and within this framework may therefore be a useful tool for predicting the age of the oceanic crust, but it cannot be used with confidence in regions of oceanic crust older than 60 m.y., where increasing age gives only a small change in elevation. At the same time, it may be possible to use the empirical correlation in the younger areas to test for jumps in the position of a ridge crest or the creation of new centres of spreading.

The model used for calculating the theoretical curve in Fig. 1 is the steady state case. Where younger phenomena are concerned it is necessary to evaluate the elevation of a ridge as a function of time. Fortunately, except for extremely slow spreading, the problem is one dimensional and the horizontal flow of heat can be ignored⁷. Thus, a ridge spreading at 1 cm/yr will move 200 km in 20 m.y. while the temperature wave travelling in the slab ahead of this section of lithosphere will be decreased in amplitude by a factor of three at a distance given approximately by $\sqrt{kt}$, where k is the thermal diffusivity and t the time. If k is equal to 0.01 cm²/s and t is 20 m.y., the distance is 25 km (dashed curve, Fig. 1). To all intents and purposes the ridge elevation drops vertically at distances greater than those corresponding to 20 m.y., which implies that the steady state case and hence the empirical profile with a vertical drop after 20 m.y. could have been used to determine the elevation on the slopes of younger ridges.

The south-west Indian ridge has long been a problem to the construction of a self-consistent pattern of relative motions for the various plates covering the surface of the globe¹⁴ and also in reconstructing the past positions of the plates in the Indian Ocean¹⁵. To the west, the ridge runs into the mid-Atlantic ridge at 45° S, 0° E and to the east it joins the central and south-east Indian ridges in a triple junction at 27° S, 65° E. Magnetic information across the ridge is sparse and, even where available, confusing¹⁶. Topographically, the ridge has much relief, though west of 60° E it seems to drop off almost vertically on

both sides from an average depth close to 3,000 m to a flat basement at roughly 5,000 m.

Fisher, Sclater and McKenzie¹⁶ have used the concepts of plate tectonics in analysing magnetic and topographic data from the central Indian ridge and have shown that the relative motion between the Somalian and Indian plates can be represented by a rotation of 6.2° per year about a pole at 16° N and 48° E. If these two plates are rotated together about this pole at a constant rate for 30–35 m.y. then the Chagos bank fits into the neck between Saya de Malha and Nazareth bank and forms a continuous feature stretching all the way from Mauritius in the south to the Laccadives in the north. This feature is at right angles to the older magnetic anomalies in the central Indian Ocean and has been named the Chagos fracture zone¹⁵. It was probably a fracture zone during an interval of rapid north–south spreading in the late Cretaceous and early Tertiary. From early Tertiary to early Miocene the spreading either stopped or was very slow. In the early Miocene, the direction of relative motion changed and spreading started along a line of weakness, the Chagos fracture zone. As this feature was not parallel to the direction of spreading, the initial break was complicated, forming the present geometry of the central Indian Ocean.

The central Indian ridge terminates to the south in a triple junction with the south-western and south-eastern Indian ridges. The south-east branch is spreading N 45° W at a half-rate of 3 cm/yr and the central Indian ridge is spreading N 60° W at a half-rate of 2.4 cm/yr. Consequently, the south-west branch is a ridge spreading N 5° W at a half-rate of approximately 1 cm/yr (ref. 15). The normal fault solution for the first motion studies on an earthquake at 27.5° S and 66° E reported by Banghar and Sykes¹⁷ supports this conclusion. Though the magnetic profiles across the south-west branch^{15,16} are confused, one line can clearly be correlated with a ridge striking N 100° W and having a half-rate of 0.7 cm/yr.

If the central Indian ridge were formed by a change in the direction of spreading in the Miocene, it is reasonable to expect that this change in direction may also account for the south-west Indian ridge. To test this hypothesis, topographic profiles across the three ridges have been analysed in further detail. Both AA', across the central Indian ridge, and BB', across the south-east branch, give a good match with the synthetic profiles. But CC', a profile lying within 20° of the direction of spreading,

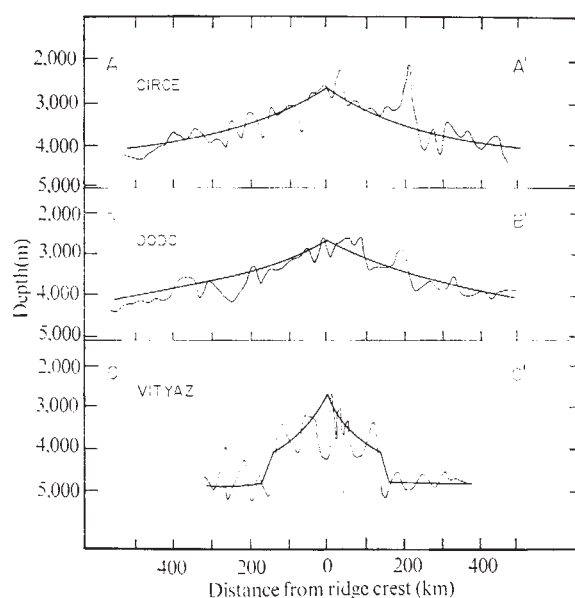


Fig. 3 Topographic profiles across the central (AA'), south-east (BB') and south-west (CC') Indian ridges. For AA' and BB' the empirical topography was determined from the age given by the magnetic anomalies. For CC' the empirical topography was computed assuming a half spreading rate of 1 cm/yr N 5° W for the past 15 m.y.

can be accounted for (assuming a spreading rate of 0.7 cm/yr) only by supposing that the ridge was formed approximately 20 m.y. ago in crust of much older age. If the patterns of relative motion between the Indian and Somalian plates and the Indian and Antarctic plates have remained constant since the early Miocene, this is evidence that the formation of the south-west branch followed some time after the breakup of the "Chagos fracture zone" and the formation of the central Indian ridge.

This reconstruction of the recent tectonic history of the triple junction in the Indian Ocean demonstrates the usefulness of analysing topographic profiles over ridge crests. This and other similar analyses^{7,18} show that the gross topography of ridges may be a powerful tool for determining their ages and hence enhance the possibility of making palaeogeographic reconstructions in areas where linear magnetic patterns are absent or poorly known.

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Observations of Unresolved Galactic X-ray Sources

We present in this article new data obtained by the OSO III satellite¹ about an apparently diffuse X-ray emission from the galactic plane. Previous observations² have established the existence of this component at a few keV and at about 100 MeV (ref. 3), but the observations reported here cover the energy range 7.7–115 keV. Theoretical speculation after the first observations has suggested several emission mechanisms—Compton scattering by cosmic ray electrons of far-infrared radiation^{4–7} or of visible photons⁸ and the presence of numerous weak discrete sources (Setti, G., and Woltjer, L., personal communication, and Ryter, C., personal communication).

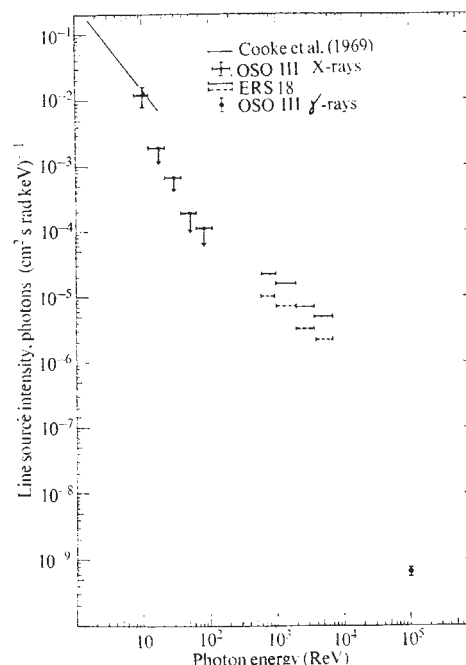


Fig. 1 Available data on galactic emission from unresolved sources. The ERS 18 detector had no collimation and therefore the results must be considered only as upper limits; the low energy and high energy directional data identify the emission with the galactic plane.

tion). Because the new data cover an energy range between the two earlier sets of observations at a few keV and 100 MeV, they impose important new constraints on the theories.

The UCSD X-ray telescope on the OSO III scanned almost the entire sky in the 14 months after launch on March 8, 1967, and produced crude maps in five energy channels between 7.7 and 115 keV. The scintillation counter telescope (22° FWHM, 10 cm²), the data reduction procedures and the measurements of the diffuse extragalactic X-ray component have all been described previously^{1,9,10}. Only in the lowest energy channel was there an excess counting rate on crossing the galactic plane in galactic longitude regions where known point sources could not contribute appreciably. This excess in the 7.7 to 12.5 keV channel was 0.20 ± 0.07 counts s⁻¹ around $l^{\text{II}} = 150^\circ$ and 0.50 ± 0.19 counts s⁻¹ around $l^{\text{II}} = 240^\circ$. Only the statistically more accurate data due to the 7 to 1 exposure ratio in the $l^{\text{II}} = 150^\circ$ region will be used here. Higher channels produced only significant upper limits. Because of the wide aperture of the telescope, neither the OSO III data nor the data² of Cooke *et al.* can exclude the possibility that a few weak discrete sources may account for the observed excess, and recent measurements (Cooke, B. A., and Pounds, K. A., personal communication) have indeed indicated that up to 90% of the previously reported flux in the 1.4 to 12.1 keV range may be due to sources in the Centaurus–Norma–Lupus complex near $l^{\text{II}} \sim 300^\circ$.

Data on the galactic X-ray and γ-ray diffuse flux are shown in Fig. 1. The OSO III data in Table 1 refer to galactic longitude $l^{\text{II}} = 150^\circ$ and are shown as 95% confidence upper limits at the higher energies. The earlier rocket result, which clearly agrees with the OSO III data, refers to longitude $l^{\text{II}} \approx 240^\circ$. Upper limits in the 0.6 to 6.0 MeV range were obtained from the ERS 18 data¹¹. The excess of counting rate in a 7.65 cm long × 6.35 cm diameter isotropic NaI scintillation counter ($G_0 = 54$ cm⁻²) over that expected from the extragalactic component according to the Brecher–Morrison model¹² was used to determine the galactic line intensities. Two assumptions about the distribution in galactic longitude were made: a uniform distribution (solid line in Fig. 1) and a distribution similar to that of 100 MeV γ-rays (dashed line in Fig. 1).

The average intensity obtained by the OSO III shown at 100 MeV is based¹³ on a revised value for the geometry factor 3.4 cm² rad, and the results agree with the new observations of Kniffen and Fichtel¹⁴.

Positive fluxes have only been observed at the extreme energies of the spectrum and the data at intermediate energies can only place limits on power law spectra of the form $J(E) \sim E^{-\gamma}$ photons (cm² s rad keV⁻¹) expected from various physical processes. The data require a value for γ larger than 3.2 for the 7.7 to 65 keV energy range and less than 1.8 for the 0.038 to 100 MeV energy range (95% confidence levels). Similarly the ERS 18 excess in comparison with the OSO III data requires an index less than 0.9 over the energy range 0.038 to 1.0 MeV. A thermal bremsstrahlung spectrum of effective temperature between 5 and 7 keV could fit all the X-ray data ≤ 65 keV.

Compton scattering of far-infrared radiation. A number spectrum of electrons $dn/dE = KE^{-m}$ cm^{-m} can Compton scatter low energy photons with a blackbody distribution at temperature T and energy density ρ eV cm⁻³ to produce diffuse X-rays of number spectrum¹⁵

$$dn/dE \sim \int_0^{r_{\max}} K \rho E^{-\frac{(m+1)}{2}} (3.6 kT)^{\frac{m-3}{2}} dr \text{ photons}$$

(cm² s keV sr)⁻¹. Cosmic ray electrons with energies > 1.5 GeV can scatter photons with wavelengths near 1 mm into the energy range of the OSO III observations. The spectrum of these electrons¹⁶ follows a power law of the form $126 E^{-2.62}$ electrons (m² s sr GeV)⁻¹. The resulting X-ray spectrum has a spectral index $\gamma = 1.81$ but, because this falls well below the lower limit of 3.2 derived from the OSO III data in the 7.7 to 65 keV energy range, we can rule out Compton scattering by these electrons as an origin for the galactic diffuse emission at a few keV. J. A. M. Bleeker and A. J. M. Deerenberg have independently reached the same conclusion (personal communication).

We can use the OSO III upper limit on galactic plane emission in the 38 to 65 keV energy range to set an upper limit on the total energy density in the far infrared, including both galactic and extragalactic sources, but we must make some assumptions about the spatial distribution of cosmic ray electrons in order to obtain a quantitative result. We consider a thickness Z perpendicular to the plane of the galactic disk to have the same uniform electron intensity as measured at the Earth; in this simple model, we would observe a "line" source $I = 16 \times 10^{-24} (3.2/T)^{0.19} E^{-1.81} \rho R_0 G$ photons (cm² s keV rad)⁻¹—the aperture correction factor⁷ $G \cong Z/R_0 (1.2 + 2 \ln \theta_0 R_0/Z)$. Our detector half angle $\theta_0 = 11.5^\circ$, and at $l'' =$

150° the line of sight distance $R_0 = 2.2 \times 10^{22}$ cm; if we assume a half thickness of 750 pc (ref. 7), we find an upper limit photon energy density of 4.3 eV cm⁻³ (a blackbody temperature of 5.5 K). By contrast, the microwave observations give a lower limit of 0.35 eV cm⁻³ for the universal radiation density.

A twenty-fold improvement in the sensitivity of X-ray observations is therefore needed to detect the Compton scattering of the cosmic ray electrons, and such observations will provide an important tool for studying the galactic distribution of the electrons.

Compton scattering of starlight. Rees and Silk⁸ have suggested that a hitherto undetected large flux of low energy cosmic ray electrons could Compton scatter the visible photons in starlight to produce galactic diffuse X-rays. We can thus place constraints on the electron intensity at around 80 MeV (with the necessary assumptions about the galactic distribution of such a cosmic ray component). We find that the 80 MeV intensity cannot exceed 3.8×10^{-5} electrons (cm² s sr keV)⁻¹ and must have a spectral index greater than 5.4 to accommodate the data in the range 7.7 to 12.5. This intensity is a factor of 3 lower than that required by Rees and Silk to produce the ~ 100 MeV γ -rays by bremsstrahlung.

Presence of discrete sources. The existence of few unresolved discrete sources provides an alternative explanation to the diffuse emission mechanisms discussed. Ögelman¹⁷ has pointed out the similarity between the galactic longitude distributions of known discrete X-ray sources and the 100 MeV line source intensity in an attempt to explain the 100 MeV γ -ray observations. The known sources, however, all have spectral indices greater than the limit of 1.8 imposed by the OSO III data at 100 MeV (ref. 18). Furthermore, γ -ray measurements at energies above 50 MeV find no point sources¹⁹ at a threshold below the average flux required.

The discrete source explanation is, however, more likely in the X-ray energy range. Although Setti and Woltjer (personal communication) and Ryter (personal communication) have shown that objects as luminous as Sco X-1 cannot have remained unresolved and still provide sufficient flux to explain the observation of Cooke *et al.*², a class of low intrinsic luminosity objects could exist. If these have an average intrinsic luminosity $\langle j \rangle$ and a uniform volume density n , then $I_{\text{line}} = (1/4\pi)n \langle j \rangle R_0 G \cong 1.5 \text{ keV (cm}^2 \text{ s rad)}^{-1}$ at energies below 12 keV. The total galactic emission is then given by $L_x \cong \pi \langle j \rangle n Z (15 \text{ kpc})^2 = 7.2 \times 10^{37} \text{ erg s}^{-1}$, a factor of 100 less than the estimated total emission by discrete sources²⁰. Our data combined with the data of Cooke *et al.* show that this component of galactic X-rays has the same spectrum as that found for many of the discrete sources.

Table 1 Galactic Plane X-rays

Midpoint channel energy [keV]	Measured quantity	$-9^\circ \leq b^{\mathrm{II}} \leq +9^\circ$ ($130^\circ \leq l^{\mathrm{II}} \leq 160^\circ$ and $220^\circ \leq l^{\mathrm{II}} \leq 240^\circ$)	$ b^{\mathrm{II}} > 60^\circ$	Count rate [photons s ⁻¹]	Galactic plane excess Point source [photons cm ⁻² s ⁻¹ keV ⁻¹]	Uniform line source [photons cm ⁻² s ⁻¹ keV ⁻¹ rad ⁻¹]
10.1	Counts	4,587	30,011	0.219 ± 0.064	$(4.9 \pm 1.4) \times 10^{-3}$	0.012 ± 0.004
	Time (s)	1,107	7,646			
	Rate (s ⁻¹)	4.144	3.925			
17.2	Counts	3,945	28,457	0.010 ± 0.028	$< 7.7 \times 10^{-4}$	$< 1.9 \times 10^{-3}$
	Time	2,228	16,162			
	Rate	1.771	1.761			
30.0	Counts	3,714	25,527	-0.010 ± 0.016	$< 2.2 \times 10^{-4}$	$< 5.5 \times 10^{-4}$
	Time	3,147	21,453			
	Rate	1.180	1.190			
51.5	Counts	3,142	16,641	-0.008 ± 0.011	$< 9.0 \times 10^{-5}$	$< 2.2 \times 10^{-4}$
	Time	4,931	25,800			
	Rate	0.637	0.645			
89.0	Counts	1,741	11,307	-0.014 ± 0.007	$< 3.2 \times 10^{-5}$	$< 8.0 \times 10^{-5}$
	Time	5,698	35,444			
	Rate	0.305	0.319			

The OSO III observations have confirmed weak X-ray emission from the galactic plane in the 7.7 to 12.5 keV range and have also provided low upper limits to such emission in the energy range 12.5 to 115 keV, thus eliminating the possibility of a straight line interpolation of the spectrum between the 3 to 10 keV X-ray range and the 100 MeV energy range. The spectral index limits for the galactic emission suggest that these X-rays may come from low luminosity discrete sources with effective temperatures of 50 to 80 million K, and that the 100 MeV photons probably do not arise solely from the Compton scattering of cosmic ray electrons. This is consistent with the recent survey by Cooke and Pounds (personal communication), who find that the previously diffuse intensity in the region $l^{II} \sim 300^\circ$ can be explained by sources which are now resolvable.

The X-ray observations yield a new upper limit of 1.7×10^{-9} W (cm² sr)⁻¹ for any universal intensity of far-infrared photons (at about 1 mm wavelength), assuming that the local intensity of ~ 3 GeV electrons is uniform throughout the galaxy. The same considerations applied to visible starlight lead to a new upper limit for the intensity of ~ 80 MeV cosmic ray electrons of 3.8×10^{-5} electrons (cm² s sr keV)⁻¹.

These X-ray observations represent a new means of exploring the spectrum and spatial distribution of cosmic ray electrons. More sensitive experiments should soon make possible the direct detection of Compton scattered radiation in the sub-millimetre or microwave spectral ranges.

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Ball Lightning as an Optical Illusion

DURING the past year there have been numerous publications on ball lightning¹⁻⁹, many attempting to account for the formation, properties and behaviour of lightning balls. None

have questioned the reality of the phenomenon, in spite of the lack of progress toward an understanding of these baffling objects. Serious doubt about the existence of ball lightning was expressed by Humphreys¹⁰ in 1936, and more recently by Schonland¹¹. Both regarded the phenomenon as probably an optical illusion. Now that Altschuler *et al.*⁸ have invoked nuclear reactions to account for the lightning ball it seems appropriate to re-examine the possibility of finding an explanation in the physiology of vision.

The phenomenon of visual afterimages is very complex, but its predominant effect—the negative afterimage—is well known. Of less frequent occurrence is the positive afterimage, which results from the observation of a light source which is very bright relative to the surround. In this communication I shall assume that a spherical region of ionized air and white hot particles is sometimes generated at the ground end of a lightning stroke by partial vaporization of Earth, vegetation or metal, and that the luminous intensity within the sphere is high enough to create a positive afterimage in the eye of an observer. In his comprehensive survey of reports on ball lightning Rayle¹² made the interesting discovery that “the number of persons reporting ball lightning observations is 44% of the number reporting observation of ordinary lightning impact points”. This can be understood by assuming that about half of all strokes to ground generate a high luminosity ball at the impact point. Indeed, this sort of assumption is required whether ball lightning is real or illusory.

The behaviour and apparent properties of the positive afterimage are strikingly similar to those of ball lightning. Its shape will be the same as that of the exciting source, and it will commonly be described as a ball. The apparent size of the positive afterimage will be the same as that of the exciting source only if it is “projected” by the observer to the distance of the source. If the source is outdoors, for example, but the observer focuses on the window through which it was seen, the apparent size will be reduced. Thus the relatively small size of indoor lightning balls is accounted for. Rayle¹² pointed out that linear size and distance were one of the few pairs of parameters that showed a significant correlation, but he did not go on to infer the implied limited range of angular diameters.

The colour of positive afterimages depends more on the brightness of the source than on its colour. There is no conflict with the yellows, oranges and occasional other colours reported for ball lightning. The degree to which the size, shape, brightness and colour of the lightning ball remains constant throughout its lifetime of a few seconds is the despair of both the theoretician^{8,13-16} who seeks a plausible physical mechanism for it, and the experimenter³ who tries to reproduce it. On the other hand, the approximate constancy of these features is characteristic of a positive afterimage.

Being a cone effect, the positive afterimage will usually be formed near the centre of the retina, and the observer, wishing to examine the supposed object, will attempt the impossible task of centring it exactly. The result is a linear drift of the projected positive afterimage across the observer's visual background, as the eye muscles try to correct the centring error. In natural circumstances this feedback motion will be combined with varying amounts of voluntary adjustment as the observer strives to make sense out of the motion.

Passage through physical surfaces such as a glass or metal screen is possible for positive afterimages and is reported for lightning balls^{3,4}. In neither case is the surface burned or damaged, nor is the size, shape, colour or brightness of the ball altered by the penetration.

Positive afterimages last 2–10 s, depending on circumstances, and most lightning balls are reported to have a duration in the same range¹². Positive afterimages disappear rather suddenly, as do lightning balls. Positive afterimages generate no sound but the typical observer finds it easy to imagine “suitable” accompanying sounds, if he has any relevant preconceptions. The ease with which most people “hear”

appropriate sounds while observing natural phenomena has been documented by Beals¹⁷, who found that most of the inhabitants of northern Canada claimed to have heard the aurora borealis. The spectrum of sounds reported was persuasively narrow and differed only moderately from observer to observer. Apparently it is no more maladaptive to hear the rustling of the northern lights than it is to hear the implosive pop of a disappearing lightning ball. Curiously, the disappearance of indoor lightning balls is reported often to be violent but seldom to be damaging.

The psychological principle for imagining odour is the same as for sound but fewer individuals have strong odour expectations and in only a few cases are odours reported. An appropriate odour would be one the observer associates with electrical apparatus, if he has some familiarity with it; or with deeper metaphysical concomitants of danger and mystery, such as fire and brimstone, if his associations tend to the supernatural. Such are the reported odours of ball lightning¹⁸.

In bright daylight positive afterimages are a rare occurrence. But in subdued light, as during a thunderstorm, especially if the observer is indoors, a positive afterimage is readily formed by any source as intense as a frosted light bulb. Considering that lightning is primarily an outdoor phenomenon it is remarkable how many lightning balls are seen indoors. This strange fact can be understood if positive afterimages are involved.

That the unsuspecting observer of a chance natural phenomenon can be misled by a positive afterimage is more than speculation. Two of the eyewitnesses to the passage of the Revelstoke bolide¹⁹ reported independently that the meteor had landed on low ground in the middle distance, and had bounced several times before going out. It is more likely that these observers each followed a positive afterimage down to zero elevation after the bolide passed behind distant mountains. At ground level a conflict between positive afterimage drift and physical anticipations led to an unstable situation in which the positive afterimage oscillated briefly before disappearing.

There are a few reports which indicate the release of large amounts of energy from the lightning ball. In the most famous of these cases, described by Goodlet²⁰, water in a rain barrel was heated by a lightning ball. If ball lightning is an optical illusion it will be necessary to categorize this and similar reports as unreliable. To do so would not be unreasonable in view of the many observations on record.

Final resolution of the question of the nature of ball lightning will no doubt depend more on the outcome of further experimentation than on the collection of more observer reports. Fortunately, the question of the reality of ball lightning is amenable to laboratory investigation. I have found it easy to simulate the lightning ball by using light bulbs, strobe-flash lamps and photographic flash bulbs as intense sources for the generation of drifting positive afterimages. This qualitative work requires verification, however. It is hoped that a laboratory equipped for psychovisual studies will take up the problem and report on the degree to which descriptions of "artificial" ball lightning resemble those of the natural phenomenon that are recorded in the scientific literature.

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Is Ball Lightning caused by Antimatter Meteorites?

ALTSCHULER *et al.*¹ suggest that ball lightning²⁻⁵ arises from a concentration of short lived radioactive isotopes produced by lightning and imply a need for radiation measurements near thunderstorms and tornadoes. We wish to report measurements of this type which have been made for about 12 months to test the hypothesis that ball lightning is caused by the annihilation of minute fragments of meteoritic antimatter from the upper atmosphere.

This speculative hypothesis attempts to explain the puzzling properties⁶ of ball lightning, in particular its ability to enter buildings and aircraft^{7,8} and its estimated energy of 10^5 J– 10^6 J. The theory depends on the postulate that antimatter can be comparatively stable in the presence of ordinary matter. Annihilation of a speck of antimatter of 5 μ m radius and 5×10^{-10} g would liberate 10^5 J; ionization produced in the surrounding air by this annihilation could be expected to have the optical properties attributed to ball lightning.

The possible existence of antimatter has long been the subject of conjecture⁹⁻¹⁴. Inconclusive attempts have been made to show the existence of meteorites composed of antimatter¹⁵⁻¹⁷. Nauenberg and Ruderman¹⁸ consider the behaviour of an antimatter meteorite in the atmosphere; in attempting to predict its behaviour they consider the much simpler reaction between atomic hydrogen and anti-hydrogen and derive a potential energy curve for this reaction which shows a repulsive barrier of 0.5 eV. Since we began our observations Morgan and Hughes have stated that no barrier exists between hydrogen and anti-hydrogen¹⁹. But the rationale of our theory and experimental observation is the possibility that a barrier exists between antimatter in the solid state and ordinary matter; thus a small particle of antimatter could be comparatively stable if its velocity relative to the surrounding air is sufficiently low that impacting air molecules cannot overcome this hypothetical potential barrier.

The following simple argument suggests how antimatter dust particles could be concentrated by thunderstorms. The dust particles will tend to charge negatively due to the emission of positrons by photo-emission and by secondary emission caused by recoiling fragments. This negative charge will give them a tendency to descend in the vicinity of thunderstorms, which continually bring negative charge from the upper atmosphere down to ground level (for example, see ref. 20).

Consider the flux of antimatter meteorites required to explain ball lightning. Rayle⁴ concludes, from a survey of observations, that ball lightning occurs with 0.1–1.0 times the frequency of ordinary lightning; this gives it a world wide frequency of 10^6 – 10^7 events per day. The energy in some ball lightning events has been estimated to exceed 10^6 J; the actual average energy of all events is not known but 10^5 J is a reasonable estimate. If only 1% of incoming antimatter produces ball lightning, then the total entering the Earth's atmosphere is 20–200 g/annum.

Pettersson²¹ estimates that the annual influx of meteoric material is 5×10^9 kg. Thus a fraction of about 10^{-11} should be anti-matter—well within estimates for the maximum fraction of antimatter in our galaxy, which range from a recent estimate by Libby¹² of 3×10^{-4} to 10^{-7} estimated by Burbidge and Hoyle⁹ and 10^{-8} by Hones¹⁰. The power equivalent to our estimated influx of antimatter is 10^9 – 10^{10} W which compares with 0.4×10^{16} W from cosmic rays²².

This theory is no more than interesting speculation; a detailed analysis of the behaviour of antimatter in the presence of ordinary matter is needed in order to decide whether ball lightning could be caused by antimatter dust. If, however, micron-sized particles of antimatter are annihilated in the vicinity of thunderstorms their presence should be detectable. One would expect to observe short (the lifetime of ball lightning is ≤ 10 s) intense bursts of both 511 keV γ -rays from positron-electron annihilation and radiation over a wide energy range from mesons and their nuclear reaction products. A following period of enhanced 511 keV radiation might also be expected from the positron emitting nuclei ^{13}N and ^{11}C produced by meson reactions and nucleon boil-off, but the intensity due to this process is unlikely to exceed 0.5% of that in the short burst.

To observe the occurrence of such short intense bursts of radiation a 20 cc Ge(Li) detector and a 900 cc NaI(Tl) detector were set up with the counting rates in four channels monitored continuously. The channels were 511 ± 50 keV and 662 ± 50 keV for the Ge(Li) detector and 511 ± 75 keV and all energies above this channel for the NaI detector. The number of counts in each channel was printed out every 15 min and the counting rates in the two 511 keV channels were monitored on a dual channel Rustrak impact recorder. In normal conditions the counting rates in the channels were approximately 0.5, 0.2, 30 and 70 c.p.s. respectively.

We have observed four unusual radiation events. Three (*A*, *B* and *C*) were recorded on June 24 and 25, 1970. The fourth (*D*) was registered on August 7, 1970, when the equipment also included a multichannel pulse height analyser arranged to analyse the NaI counter output should the rate in the NaI 511 keV channel exceed 400 c.p.s.

In each of the four events both detectors showed a large transient increase of counting rate. For events *A*, *B* and *C* the ratio of the estimated number of counts in the 511 keV channels due to the transient burst agrees to within $\pm 50\%$ with the ratio of the measured photopeak efficiencies ($\sim 150:1$) of the two detectors for annihilation radiation. In event *D* the variation of the background prevents the same estimate from being made. These four events are the only occasions when simultaneous enhancements occur.

Fig. 1 shows the chart recordings of the events. The ratemeter time constants were set to 25 s for events *A*, *B* and *C*; consequently the chart record only allows an upper limit of ~ 10 s to be placed on the duration of the events. In event *D* the NaI 511 keV channel ratemeter had a time constant of 1 s; the six impact points on the chart record indicate that its duration was less than 3 s. Beneath each event the integrated counts in the 511 keV NaI channel are shown.

In event *D* the multichannel analyser was switched on for 10 min; Fig. 2 compares the pulse height spectrum obtained with a "normal" spectrum. The peak contains 7,700 counts centred at 540 keV with an FWHM of 126 keV (which corresponds to the resolution of the detector) and is entirely consistent with that from annihilation radiation. During event *D* the counting rate in the >511 keV NaI channel correlated closely with the time distribution of the rainfall recorded nearby and was probably caused by washed down $^{214}\text{Bi}(\text{RaC})$ from the radon decay chain as reported elsewhere²³. The behaviour of the 511 keV NaI channel was quite different and cannot be ascribed to the wash down of radioactivity; it is consistent with the presence of ^{13}N and/or ^{11}C .

To determine whether the events were correlated with thunderstorm activity the weather records from the Meteorological Office at Abingdon (eight miles from AERE) were used.

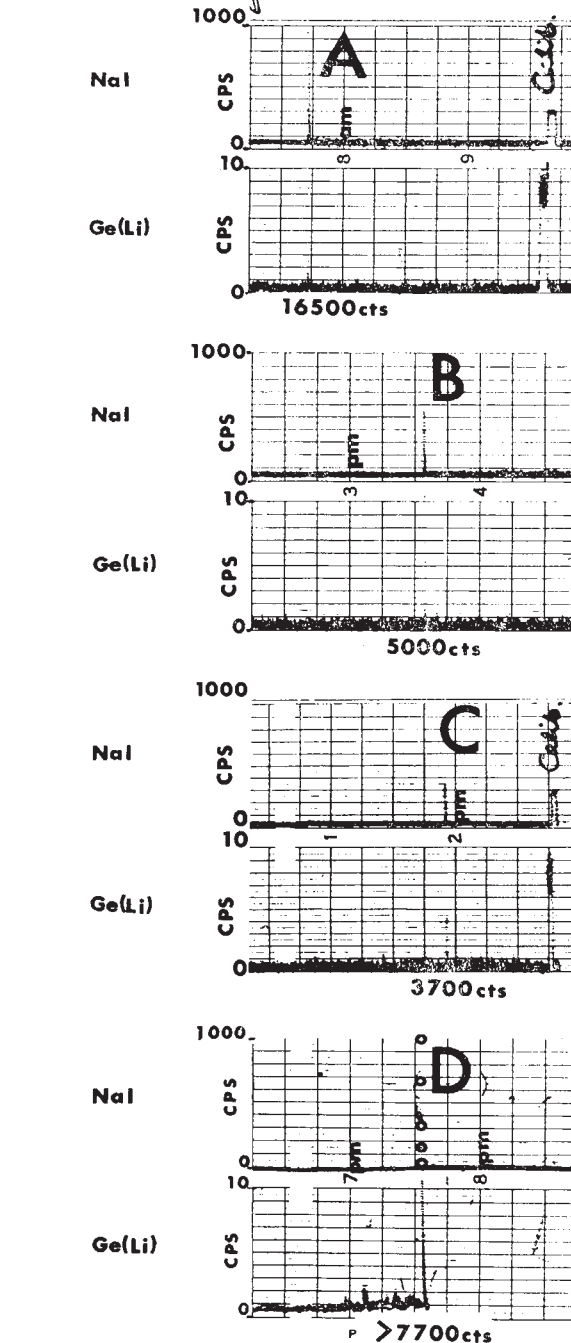


Fig. 1 Chart records of the four events showing the counting rates of the 511 keV channels in both detectors. In event *D* the circles surround the six single impact points of the record. In *A* and *C*, calibration peaks are also shown.

In the observation period of 151 days, five thunderstorms were observed at Abingdon and there were seventeen thunderstorm warnings. Event *D* occurred at the height of a severe thunderstorm with exceptionally high rainfall (65 mm in 1 h). The other three events occurred on two consecutive days when there were neither thunderstorms nor warnings reported from Abingdon. These events occurred 4 days after an exceptionally intense period of thunderstorm activity (eight warnings and five thunderstorms in 13 days). From these four events it is not possible to conclude what correlation exists with thunderstorms.

To conclude, we have observed four events where the counting rate in the 511 keV region increased by factors of ten to fifty for periods of a few seconds in two independent

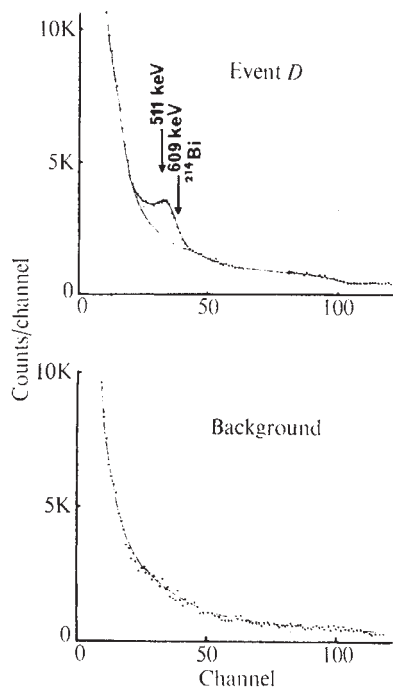


Fig. 2 Spectrum obtained from the NaI detector in 10 min when event *D* triggered the pulse height analyser. The lower spectrum is that due to normal conditions.

counters; these rates are equivalent to those produced by a 1 mc source of 511 keV radiation immediately outside the hut containing the detectors and would correspond, for example, to the annihilation of between 10^{-11} g and 6×10^{-11} g of antimatter at 500 m distance. In one event a pulse height distribution was obtained with a peak apparently due to annihilation radiation. These events do not support the ball lightning theory of Altschuler *et al.*¹; in particular the duration of 511 keV radiation for a few seconds is inconsistent with the half life of ^{15}O and ^{17}F (123 s and 66 s respectively) which they suggest might be associated with ball lightning. The radiation events are consistent with the proposed existence of micron-sized particles of antimatter and their duration is similar to that of ball lightning; however, the correlation with thunderstorms is uncertain. Obviously the results neither prove the existence of antimatter dust particles nor their association with ball lightning; they are presented to stimulate further observations and re-examination of existing radiation records for similar transient effects.

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BIOLOGICAL SCIENCES

Meiotic DNA Synthesis

A MAJOR episode of DNA synthesis occurs during the interphase preceding meiosis¹⁻⁴. Additional periods of synthesis have been detected by autoradiography during leptotene, zygotene or pachytene in newt⁵, man⁶ and mouse⁷. In *Lilium* and *Trillium* separate periods of synthesis have been demonstrated at zygotene and pachytene by ³²P-phosphate labelling followed by DNA extraction⁸. Others have failed to detect prophase synthesis autoradiographically^{3,4}, presumably because of the low level of label incorporation during synthesis of amounts of DNA estimated to represent 0.3-2.0% of the total nuclear DNA content^{5,9}.

We report here the incorporation of tritiated thymidine into chromosomes occurred at most, and possibly all, stages of meiosis. We used the cultivated wheat (*Triticum aestivum* variety 'Chinese Spring' $2n=6x=42$) and an understanding of the results requires some knowledge of the duration of meiotic stages in this genotype. Table 1 shows that in wheat anthers at 20° C the total duration of meiosis from leptotene to second telophase is 24 h (Bennett, Chapman and Riley, unpublished). Leptotene (10.4 h), zygotene (3.4 h) and pachytene (2.2 h) are the three longest stages, while diakinesis (0.4 h) and first and second anaphase and telophase (all 0.5 h) are the shortest stages. Meiosis is much faster in wheat than in any other species in which meiotic DNA synthesis has been studied by isotope labelling. For example, at 20° C meiosis lasts for 9-10 days in the anthers of *Lilium* and for 11-12 days in *Trillium*¹⁰, while in mouse at body temperature it lasts 13-14 days⁴. Indeed, meiosis is as fast in wheat as in any other higher organism in which it has been timed.

The three anthers in each floret of wheat are approximately synchronous in the stage of meiosis attained by their pollen mother cells. Consequently, the meiotic stage of one anther can be determined and the other two exposed to experimental treatment with reasonable assurance that they are at the same stage as the tested anther.

The stimulus for our work was the observation that ³H-thymidine was incorporated in chromosomes during late meiotic prophase in excised wheat anthers after culture for 1 h in the presence of ³H-thymidine, followed by 5 h of culture without the isotope. To examine this situation further, wheat anthers with pollen mother cells at many stages, from immediately pre-leptotene to second telophase, were placed in fresh White's medium supplemented with 2% (w/v) sucrose and

containing ^3H -thymidine (20 $\mu\text{Ci/ml.}$, specific activity 5 Ci/mmol, Radiochemical Centre, Amersham). After 1 h or 1.5 h in the medium anthers were fixed in acetic-alcohol, taken through 95% and 70% alcohol to water, hydrolysed in 1 N HCl at 60° C for 12 min and stained with leuco-basic fuchsin. Autoradiographs of anthers squashed on microscope slides were prepared by dipping in Ilford Nuclear Research Emulsion K2, diluted with water (1:1). The slides were air-dried, then exposed for 200 days at 4° C after which they were developed in Kodak D-19 developer.

Where there were differences between the meiotic stages attained by the initial test anther, sampled from each floret, and those attained by the cultured anthers, they showed that in culture meiosis had proceeded at the pace normally expected at 20° C. The meiotic stages present in the autoradiographs were those that had been attained in the presence of ^3H -thymidine which had been available for either 1 or 1.5 h. In spite of this, chromosomes were labelled in pollen mother cells at all stages of meiosis including leptotene, zygotene, pachytene, diplotene, diakinesis, first metaphase anaphase and telophase, dyads, and second metaphase, anaphase and telophase (Fig. 1). Consequently DNA synthesis occurred throughout meiosis. Table 1 shows that labelled first metaphase cells were unlikely to have been at a stage earlier than diplotene when first exposed to ^3H -thymidine. Similarly, labelled first anaphase cells were not earlier than first metaphase when initially exposed. Labelled second division cells would have passed through first division before ^3H -thymidine was supplied. There were no major distinctions in the extent of labelling in meiotic stages with contracted chromosomes, although pachytene sometimes seemed to have more silver grains, but in the dyad and tetrad stages, with despiralized chromosomes, there were fewer grains per nucleus.

Some details of the distribution of silver grains in the autoradiographs must be mentioned. (1) At first anaphase, when the two chromatids of a chromosome were spread apart in squashing, silver grains were sometimes present at similar positions on both sister chromatids (Fig. 1H). (2) At first metaphase silver grains occasionally occurred in corresponding regions of the partner chromosomes of a bivalent (Fig. 1F and G). (3) At first anaphase there was differential distribution of silver grains over chromosomes at the same pole. When anaphase chromosomes, formerly in the same bivalent, could be matched across the plate there was sometimes a correspondence in the degree of labelling in homologous chromosomes (Fig. 1I).

The incorporation of ^3H -thymidine demonstrated that DNA synthesis occurred at many meiotic stages in the system studied. It might be argued that the experimental conditions—excision of the anther, the culture medium or the presence of the isotope—generated abnormal synthesis that does not take place *in situ*. There is no evidence to refute this, but it seems unlikely since label incorporation presumably required the presence of a DNA polymerase and ligase and it occurred during meiotic stages, such as metaphase and anaphase,

when DNA transcription probably ceases¹¹ so that the enzymes, or appropriate long lived messenger RNAs, must already have been present in the cell. This could be interpreted as meaning that, since the necessary components of the synthetic system are available, DNA synthesis normally occurs at these times. We do not wish to overemphasize this argument but it does suggest that the ^3H -thymidine incorporation that we observed was not abnormal.

The normal occurrence of DNA synthesis late in meiosis might be due to delayed replication in regions in which synthesis was not completed in the previous interphase or early prophase^{8,9}. Alternatively, it might result from the continual repair of defects. The observation of similar levels of synthesis in homologous chromosomes suggests delayed replication at specific sites, and a similar interpretation could be placed on incorporation at corresponding sites on sister chromatids. An alternative explanation for the matching synthetic regions on chromatids might be that it is the consequence of sister chromatid crossing over, and such crossing over may result from simultaneous repair at corresponding loci.

There is no basis for a choice between the alternatives of late replication or the repair of lesions, but if we were detecting repair it would imply that the DNA structure is constantly monitored during meiosis and defects corrected. This would not be surprising except that meiosis is a process which usually progresses to its termination irrespective of abnormalities of chromosomal behaviour that may arise. On the repair hypothesis, it is only errors in the movement of the chromosomes or in their structural organization that are not corrected, for molecular integrity of the DNA is maintained. The possibility that the rate of repair was enhanced by the occurrence of damage induced by the radioactivity¹² must be recognized but additional repair might also be necessary during meiosis because of damage that can be postulated to arise from chromosome coiling.

A further implication of these observations is that DNA replication or repair takes place even when the chromosomes are supercoiled and at their maximum contraction. Apparently the structural organization of the chromatin does not preclude DNA synthesis, or perhaps synthesis takes place on the surface of the chromosomes. It is therefore possible that synthesis similar to that at meiotic metaphase and anaphase takes place at mitotic metaphase and anaphase.

It is often held that meiotic recombination involves some DNA synthesis, and synthesis has been observed at pachytene^{5,8} the stage at which recombination is thought to occur. If the DNA synthesized during pachytene is not different from that synthesized at other meiotic stages, recombination may not depend primarily on the capacity of the cell to undertake synthesis. The critical features of the nucleus that may be held to be of crucial significance in recombination would then be the relative positions of homologous chromosomes—perhaps it is essential that they are held in the jig of the synaptonemal complex—and their state of coiling. When these conditions are attained the DNA synthesis that is taking place in any case can lead to recombination.

We have discussed ways in which ^3H -thymidine could be incorporated into the linear continuity of the chromosomal DNA late in meiosis. Alternatively, the incorporation might indicate a limited replication of DNA which is not destined to be transmitted to the nuclei of the cells derived from the division. This would imply the production of short lived DNA having a structural function, perhaps concerned with chromosome coiling, or a messenger function like the so-called I-DNA¹³.

Why has DNA synthesis late in meiosis not been recognized previously? As with many issues of this nature, the answer seems to lie in the favourable nature of the organism used. Wheat has a very rapid meiosis so that exposure to ^3H -thymidine for quite a short period encompasses a considerable period of meiotic synthesis. (After allowing for the duration of meiosis and DNA content per nucleus, 1 h of meiosis in

Table 1 Duration of the Meiotic Stages of *Triticum aestivum* Variety 'Chinese Spring' at 20° C.

Stage	Duration (h)
Leptotene	10.4
Zygotene	3.4
Pachytene	2.2
Diplotene	0.6
Diakinesis	0.4
Metaphase 1	1.6
Anaphase 1	0.5
Telophase 1	0.5
Dyads	2.0
Metaphase 2	1.4
Anaphase 2	0.5
Telophase 2	0.5
Total	24.0

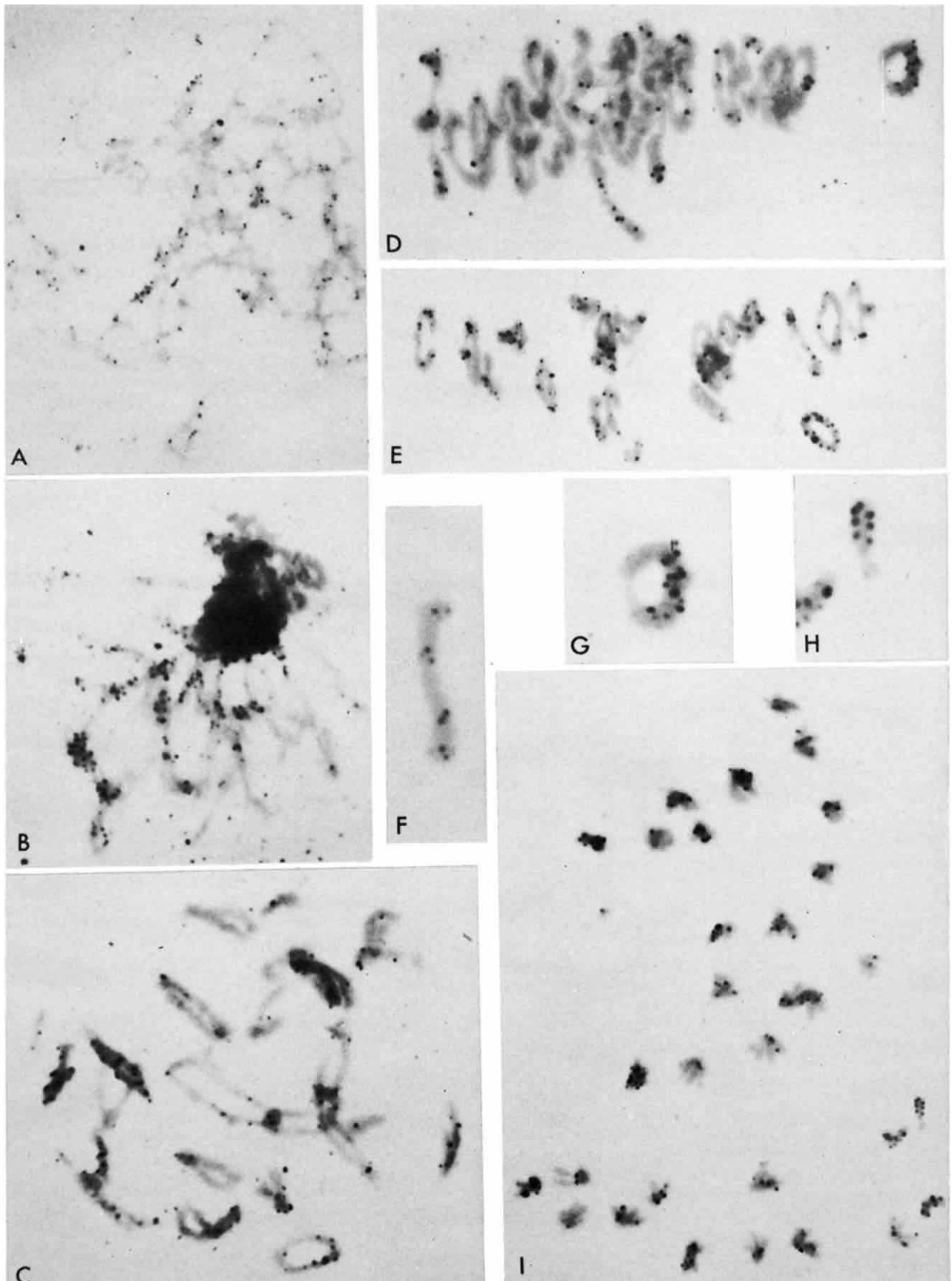


Fig. 1 Photomicrographs of autoradiographs of anthers given a 1 h pulse of ^3H -thymidine followed by immediate fixation. Labelled meiotic chromosomes are shown at (A) late zygotene, (B) mid-pachytene, (C) diplotene, (D and E) first metaphase and (I) first anaphase. At first metaphase, silver grains sometimes occurred in corresponding regions of the partner chromosomes of a rod (F) or a ring (G) bivalent. Unequal distribution of silver grains between bivalents occurred at first metaphase and first anaphase (E and F). At first anaphase homologous chromosomes, formerly in the same bivalent, could sometimes be matched across the plate by their corresponding degree of labelling and relative position, for example, the heavily labelled chromosome at the extreme left at either pole in (I) (this plate does not include the complete 42 chromosome complement). (H) shows a chromosome at first anaphase, enlarged from (I), in which silver grains were present at similar positions on both sister chromatids.

wheat corresponds to 5 h in lily.) Another factor was the long exposure of the autoradiographs, which possibly allowed the detection of lower levels of incorporation than is usual in similar experiments.

Of course we realize the crucial importance of demonstrating that the ^3H -thymidine incorporation that we observed is indeed into DNA. This is being tested, but we considered that the observations were of such significance that we should publish them before our inference had been proved beyond doubt because of the time that this proof will take. We are extracting DNA from meiotic cells at appropriate stages of meiosis after exposure to ^3H -thymidine, but we must ensure that there is no contamination from the DNA of surrounding somatic tissue, and techniques are not yet sufficiently precise for this. Until a biochemical procedure of this type is available it will be difficult to assess the amount of DNA synthesis that occurs during meiosis relative to the total DNA content of the nucleus.

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Nuclear Transplantation in *Drosophila melanogaster*

It has been known for some time that factors in the cytoplasm of many eggs can influence the development of the cells, part of which they eventually constitute. This is most clearly seen in the case of the germ cell determinants. The removal of a particular region of the egg (the "pole plasm") results in the absence of germ cells in the resulting animals¹. We have begun an analysis of nuclear and cytoplasmic interactions in the egg of *Drosophila melanogaster* by means of nuclear transplantations. The advantages of *Drosophila* are its well described genetics and development; its disadvantages are the small size of the egg, its extreme sensitivity to minor damage to the egg membranes and the high turgor of the egg cell. Although nuclear transplantations in other animals have been fruitful², two earlier attempts with *Drosophila*^{3,4} yielded a few differentiated larvae, but no adults. This was probably because injected eggs usually develop abnormalities which lead to early death of the embryo. We have tried to bypass this difficulty by culturing early embryonic stages in adult females and then causing them to metamorphose, a procedure developed independently by Hadorn *et al.*⁵ and Gateff and Schneiderman⁶. This made it possible to assess the developmental capacities of embryos from nuclear transplantations which underwent a great deal of development but would never have hatched.

Several mutants of *Drosophila* were used as donors of the preblastoderm nuclei. These include *yellow* (y ; 1-0.0, pale bristles and cuticle); *yellow, white* (eyes) *singed* (gnarled bristles)

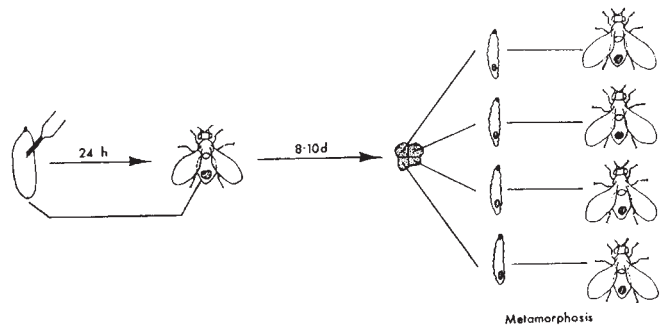


Fig. 1 Schematic drawing of the culture method for nuclear transplants. Transfer of preblastoderm nuclei into the host egg; after 24 h the developing embryo is cultured in an adult female for 8-10 days. The retrieved implant is then fragmented and transplanted into mature larvae, where the pieces metamorphose simultaneously with their hosts. To enhance the production of unfertilized eggs we mated virgin wild type females with sterile XO males. Such males were obtained by crossing attached XX females from an attached XX/XY stock ($y^2 su(w^d) w^a bb/v f B, XY$) with wild type males. All males in the following generation were XO. Samples from collections of unfertilized eggs were kept for several days and checked to be sure no larvae developed. Larval and adult hosts in which the embryos were cultured were all of the wild type strain Oregon-R.

(w ; 1-1.5; sn^3 ; 1-21.0); and *ebony* (dark cuticle) *multiple wing hairs* (two to seven rather than one trichome per cell) (mwh ; 3-0.0; e^{11} ; 3-70.7). Recipients of injected nuclei in different experiments were eggs of wild type Oregon R, y ; y , w , sn^3 or e^{11} , mwh stock. The procedure is described in the legend to Fig. 1. For cytological observations injected eggs were fixed in ethanol-acetic acid and stained by the Feulgen method.

In the first series of experiments we transplanted nuclei and cytoplasm from preblastoderm stages of various genotypes into fertilized host eggs during the nuclear cleavage stage. Of 473 injected embryos, 106 showed progress in development when examined on the following day; these were transferred to adult hosts (Table 1). Many had reached the stage of segmentation and frequently also contracted. In some cases, intestine, Malpighian tubules, trachea and mouth hooks were evident. Six embryos had developed so far that they were not dissected from the egg but were kept in small food vials and were permitted to hatch. All hatched, completed larval life and pupated; five flies emerged, all showing chitinous structures exclusively from the host genotype. Although these results do not provide any information on the fate of the transplanted nuclei, they show that injected embryos survive the experimental manipulations and can metamorphose.

Largely because of infection, a high percentage of the adult flies which received implants died during the culture period. Forty-three flies survived, from which thirty-eight implants were retrieved (Table 1). These implants resembled the cultured embryos and embryo halves described by Hadorn *et al.*⁵. To test the genotype and the developmental capacities of the injected nuclei, implants derived from cultured embryo fragments were put into mature larvae, where they metamorphosed together with the host. Frequently, the implant had grown so large that it had to be divided into several fragments for successful larval transplantations.

As Table 1 shows, embryos derived from twenty-two eggs which had received nuclear implants gave rise to adult differentiations. In twenty of these cases, various imaginal structures of the host genotype were produced. In two cases mosaic implants containing both host and donor genotype were found. In the first mosaic, wild type host structures included derivatives of the eye-antenna, labial, leg and wing disks. Yellow structures derived from donor nuclei included derivatives of the wing-thoracic and haltere disks. The second mosaic consisted of e^{11} , mwh head structures derived from the host and a small vesicle of wing blade derived from injected nuclei carrying the gene y .

Table 1 Summary of all Nuclear Transplantations

Type of host	No. of eggs injected	No. of transferred embryos	No. of flies after 8-10 day adult culture		Dead	No. of cases after metamorphosis			
			with implant *	without implant		With adult structures *	Without implant	With residual body	Where all larval hosts died
Fertilized cleavage stage	473	106	38	5	63	22	9	2	5
Unfertilized	780	94	21	55	18	6	2	10	3

* Each of the cases is based on several metamorphosed fragments derived from a single injected embryo.

In a second series of experiments we used unfertilized host eggs. This kind of experiment has the advantage over the one just described in that successful nuclear transplants can be detected on the following day. We therefore concentrated on this type of transplantation.

Because we were not able to observe the nuclei during transplantation we determined the approximate number of injected nuclei by Feulgen staining smears of unfertilized host eggs immediately after injection. Fifty-two eggs were examined. The average number of nuclei was seven, some eggs containing no nuclei, others up to twenty and in one case thirty.

Of 780 nuclear transfers into unfertilized eggs, ninety-four showed enough development to merit transplantation into adult hosts (Table 1). We made no histological preparations of these embryos, and so cannot provide detailed information on the exact stage of development they reached before adult culture; but their development was far less advanced than was the case with fertilized host eggs. Most of the ninety-four transferred embryos seemed to be at least cellularized and held together quite firmly, but had no further distinguishable differentiation. Nonetheless, we found some embryos in the first stages of segmentation.

After adult culture many of the implants could not be retrieved. We believe that this loss occurred either because embryonic development was already abnormal and the tissues died soon after transplantation, or because the embryos were too young and therefore difficult to culture by this method. When implants were recovered, some of them showed distinctive larval structures such as intestines, Malpighian tubules and imaginal disks. The implants were then caused to metamorphose.

Six nuclear injections led to the formation of chitinous adult structures (Fig. 2), all of the genotype of the transplanted preblastoderm nuclei. We found metamorphosed structures derived from the eye-antenna, leg, wing and haltere disks and possibly also from the abdominal histoblasts.

After metamorphosis we frequently found masses of melan-

ized material which has been called residual body⁷. The presence of this material indicates that the transplanted tissue was not healthy.

The following conclusions can be drawn. Development of unfertilized eggs of *Drosophila melanogaster* can be initiated by injected preblastoderm nuclei. Moreover, transferred preblastoderm nuclei can integrate into a normal embryo of another genotype. The method can be used to test the morphogenetic roles of cytoplasmatic factors, as the capacities of nuclei from different tissues. It also permits the injection of isotopes for autoradiographic studies of nucleic acid and protein synthesis in early embryonic development.

Nuclear transplantations were performed under paraffin oil, with the aid of a Leitz dissecting microscope and a Leitz micromanipulator, using 'Pyrex' micropipettes with a tip diameter of 9-11 μ connected by air filled tygon tubing to a 50 ml. syringe. The eggs were collected on small Petri dishes (containing 1-2%, 2 ml. acetic acid and 4 ml. 95% ethanol per 100 ml. agar solution) over 30 min at 25° C. The first collection was discarded to eliminate older eggs from the population. The eggs were then washed and sterilized in 0.05% HgCl₂ in 70% ethanol for 12 min. The eggs were then rinsed several times in sterile distilled water, dried on a microscope slide and covered with paraffin oil. At the time of transplantation the donor eggs were 100 $\pm$ 20 min old and in the preblastoderm stage. The host eggs were 40 $\pm$ 20 min old and, in the case of fertilized eggs, in the cleavage stage. Nuclei and cytoplasm from the donor egg were sucked into a micropipette. The host egg was injected in its lateral side in about the middle third of the egg. After the transplantation the injected eggs were kept under paraffin oil at 18° C overnight. The following day they were examined at a magnification of $\times$ 64. Those eggs which seemed to have proceeded in their development were picked out and washed several times in sterile Ringer solution; the egg membranes were then removed with forceps. The embryonic material was transplanted into 2 day old female flies. After 8-10 days of culture the tissue was retrieved and implanted into mature third instar larvae for metamorphosis. After metamorphosis fragments of adult tissue were recovered and the chitinous structures mounted in Gurr's water-mounting medium. This work was supported in part by grants from the American Cancer Society and from the National Science Foundation.

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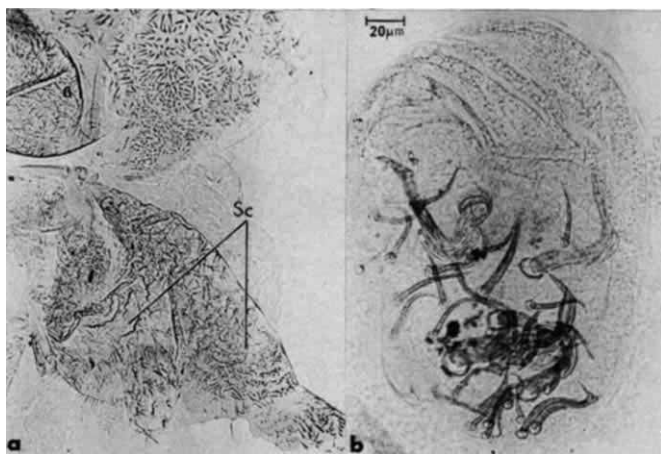


Fig. 2 Adult differentiations from nuclear transplants into unfertilized eggs. *a*, Haltere, donor genotype: *e*¹¹ *mwh*. Sc: sensilla campaniformia of the pedicellus. *b*, Thorax, donor genotype: *y w sn*³.

Pre-emption in Immunity

IN a series of experiments designed to explore the cellular basis of immunological tolerance¹, it was discovered that the response of the draining lymph nodes of mice to a subcutaneous injection of sheep red blood cells, was reduced by a slightly previous or simultaneous injection of the same antigen into the peritoneal cavity. This phenomenon will be termed "pre-emption", in the sense that the intraperitoneal injection "appropriates or takes beforehand"² the capacity to make a local immune response on subsequent subcutaneous injection. This is a descriptive term, pending the invention of a better one based on a clearly understood mechanism.

Ten to fifteen-week old male mice of the CBA strain were used. Washed sheep red blood cells (SRBC) and horse red blood cells (HRBC) were used as antigens. Intraperitoneal injections except when stated otherwise were of 5×10^8 cells in 0.2 ml. physiological saline. Subcutaneous injections were of 5×10^7 cells in 0.2 ml. saline distributed equally to four places, the two fore-paws and two sites high up on the back. The intention with these subcutaneous injections was to stimulate the internal and external axillary lymph nodes on both sides of the mouse.

Direct and indirect haemolytic plaque assays were performed on pooled cell suspensions from the four axillary lymph nodes of each mouse. Basically, the methods of Jerne, Nordin and Henry³ and Dresser and Wortis⁴ were followed but no basal agar layer was used. The developing antiserum used was a rabbit-anti-mouse-serum-protein antiserum with an inactivation constant of 0.385 (ref. 5). This serum was stored at -40°C and complement was inactivated by heating to 56°C for 30 min immediately before use. No less than five mice were used to obtain any mean value.

Plaque counts from the four pooled lymph nodes of individual mice were transformed to logarithms and the geometric means thus obtained were compared by using Student's *t* test. On all occasions the numbers of cells per four lymph nodes were determined and the numbers of plaque forming cells per million lymph node cells were calculated but in no instance did the use of this estimate give any information not already implicit in the PFC count per four lymph nodes, which is, as a consequence, the statistic quoted.

The experiments performed may be considered in four main groups, all related to pre-emption, as follows. In the preliminary experiments, the shape of the primary immune response following intraperitoneal injection of 5×10^8 sheep red blood cells into CBA mice, as quantified by Jerne plaque counts has been adequately documented by Wortis, Taylor and Dresser⁵. The response following a first and second injection of 5×10^7 SRBC subcutaneously (which was chosen as the dose which gave maximal response in the draining lymph nodes with minimal stimulation of the spleen) with a twenty-one day interval left between the two injections is shown in Table 1. The response to a single injection peaked at 5 days and approximately the same numbers of 19S and 7S plaque producing cells were present at that time. The response to a second injection was much sharper and consisted of mainly 7S plaques. On the basis of these results it was decided to select the fifth day after a subcutaneous injection as the standard time of observation in the pre-emption experiments.

Further experiments tested the duration and specificity of pre-emption. The effect on the immune response in the draining lymph nodes of varying the time interval between the pre-empting intraperitoneal injection and the test subcutaneous injection is illustrated in Fig. 1. The response to the test injection was always reduced. Pre-emption increased in severity for 4 days and remained at about the same level until after the twelfth day. From this time a recovery of response in the lymph nodes was apparent but the quality of the antibody produced was indicative of a secondary rather than a primary reaction. When HRBC were used as the pre-

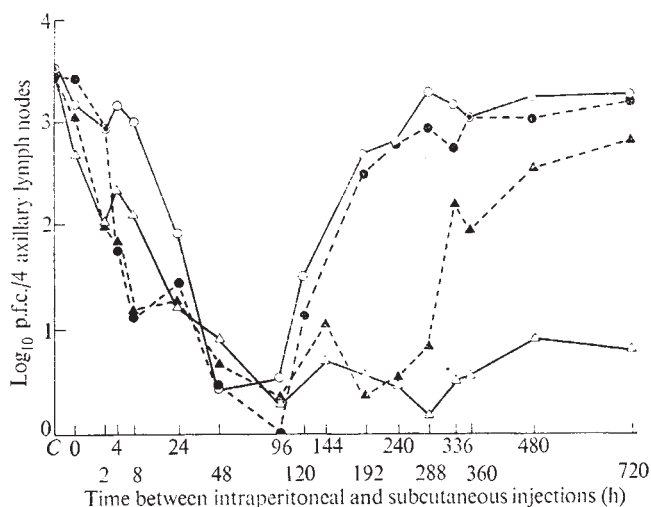


Fig. 1 Geometric means (expressed as logarithms to the base ten) of the numbers of "direct" and "indirect" plaque-forming cells per four (axillary) draining lymph nodes of CBA mice five days after receiving a subcutaneous (test) injection of SRBC at various times subsequent to an (pre-empting) intraperitoneal injection of either SRBC or HRBC. For the sake of simplicity standard deviations are not included on the graph. Control mice (C) were given no pre-empting injection. The abscissa is plotted on a square root scale for convenience of presentation. $\triangle-\triangle$, "Direct" plaques; $\triangle-\triangle$, "indirect" plaques after intraperitoneal and subcutaneous SRBC. $\circ-\circ$, "Direct" plaques; $\bullet-\bullet$, "indirect" plaques after intraperitoneal HRBC and subcutaneous SRBC.

empting antigen, it could be seen that pre-emption against SRBC was still obtained but that its development was less abrupt during the first 8 h and recovery was observed after 4 days. On this occasion the response to the test injection in the recovering lymph nodes was of a primary type but somewhat small.

To test the antigen-dose dependency of pre-emption, various numbers of SRBC were administered as a pre-empting injection and the magnitude of pre-emption was determined by a single test injection of 5×10^7 SRBC subcutaneously 2 or 4 days later. As few as 5×10^5 SRBC given intraperitoneally had a pre-emptive effect (Table 2).

Spleen dependency of pre-emption was determined by splenectomizing mice 3, 24 and 72 h after a pre-emptive injection of 5×10^8 SRBC or HRBC. At the time of removal of the spleen a test injection of SRBC was given subcutaneously. It can be seen from Table 3 that removal of the spleen diminishes

Table 1 Plaque-forming Cells in Draining Lymph Nodes after Subcutaneous Injection of SRBC

Day after first injection	Day after second injection	"Direct" plaques	"Indirect" plaques
3		0.00 ± 0.00	0.00 ± 0.00
4		2.52 ± 0.29	1.98 ± 0.33
5		3.53 ± 0.09	3.45 ± 0.17
7		2.64 ± 0.19	3.19 ± 0.16
10		2.28 ± 0.23	3.31 ± 0.27
21		0.25 ± 0.50	1.85 ± 0.10
	2	2.41 ± 0.31	3.21 ± 0.29
	3	2.44 ± 0.15	3.36 ± 0.21
	4	2.73 ± 0.10	3.07 ± 0.10
	5	1.75 ± 0.31	3.09 ± 0.19
	7	1.29 ± 0.29	2.59 ± 0.05
	9	1.42 ± 0.48	1.69 ± 0.31
	12	0.84 ± 0.13	1.59 ± 0.21
	15	0.45 ± 0.54	0.45 ± 0.54
	21	0.43 ± 0.48	0.65 ± 0.73

Geometric means $\pm$ s.d. (both expressed as logarithms to the base ten) of the numbers of "direct" and "indirect" plaque-forming cells per four (axillary) draining lymph nodes of CBA mice at various times after subcutaneous injections of 5×10^7 SRBC.

Table 2 "Direct" and "Indirect" Plaque-forming Cells in Draining Lymph Nodes 5 Days after Subcutaneous SRBC

No. of SRBC given as pre-empting injection	Interval between pre-empting and test injections (days)	"Direct" plaques	"Indirect" plaques
C		3.53 ± 0.09	3.45 ± 0.17
5×10^5	2	2.71 ± 0.19	3.06 ± 0.08
5×10^6		2.25 ± 0.09	1.88 ± 0.17
5×10^7		0.19 ± 0.39	0.71 ± 0.83
5×10^8		0.90 ± 0.67	0.65 ± 0.47
5×10^5	4	2.56 ± 0.23	2.96 ± 0.32
5×10^6		1.97 ± 0.25	2.10 ± 0.53
5×10^7		0.33 ± 0.66	0.41 ± 0.51
5×10^8		0.26 ± 0.52	0.33 ± 0.65

Geometric means $\pm$ s.d. (both expressed as logarithms to the base ten) of the numbers of "direct" and "indirect" plaque-forming cells in the four (axillary) draining lymph nodes of CBA mice 5 days after a subcutaneous (test) injection of SRBC.

the pre-emptive effect in the early stages after a pre-emptive injection but has no effect later.

A number of authors have described experiments in which simultaneous or consecutive injections of two different antigens have resulted in diminution of response to one of the antigens⁶⁻¹⁰. In some cases when the injections have been separated in time it is the response to the second injection which has been diminished. This phenomenon is referred to as antigenic competition. Liacopoulos, Merchant and Harrell¹¹ have also shown diminution in the capacity to make a graft-versus-host reaction in cells already responding to antigenic stimulation which may be a phenomenon cognate with antigenic competition. It has been found that in certain circumstances simultaneous injection of two antigenic materials leads to an increase in the immune response. This is referred to as an adjuvant effect¹². Although there are many theories as to the mechanism of these two effects there is no widely accepted explanation of either. The present experiments seem to conform to the pattern of antigenic competition except that they involve diminution of immunological responsiveness to the same as well as to a different antigen.

Table 3 Plaque-forming Cells after Splenectomy

Pre-empting antigen	Time between pre-empting and test injections	Splenectomy	"Direct" plaques	"Indirect" plaques
SRBC	3 h	+	3.10 ± 0.11	2.67 ± 0.25
HRBC		+	2.18 ± 0.18	3.20 ± 0.26
SRBC		Sham	1.48 ± 0.21	0.35 ± 0.40
SRBC	24 h	+	2.08 ± 0.12	2.42 ± 1.56
HRBC		+	2.27 ± 0.33	1.94 ± 0.54
SRBC	3 days	+	0.35 ± 0.60	0.62 ± 0.28
HRBC		+	0.65 ± 0.45	0.39 ± 0.47
C		+ day -7	3.20 ± 0.55	3.32 ± 0.27

Geometric means $\pm$ s.d. (both expressed as logarithms to the base ten) of the numbers of "direct" and "indirect" plaque-forming cells in the four (axillary) draining lymph nodes of CBA mice five days after a subcutaneous (test) injection of SRBC. In some animals splenectomy was performed at the time of the test injection.

It is recognized that when heterologous erythrocytes are administered to mice by either the intravenous or intraperitoneal routes the resulting immunological response takes place largely in the spleen^{3,13}. Few antibody producing cells are found at other sites¹⁴ (with the exception of the parathyroid lymph nodes which are slightly stimulated after an intraperitoneal injection) although there may be a slight rise in the plaque count later¹⁴. Thus a pre-empted response measured in the lymph nodes can be thought of as affected by the response to the pre-empting antigen occurring in the spleen. The failure of response in the lymph nodes could on

this oversimplified basis be thought to result from sequestration in the spleen of those cells capable of responding to sheep erythrocytes. On this basis, however, it is difficult to see why in the first 4 days after the pre-empting injection the response to horse erythrocytes is also diminished. It is commonly supposed that in relation to primary immunization sheep and horse erythrocytes are non-cross reacting antigens⁷ and consequently the cell populations which react to these two antigens might be supposed to be independent of each other. The pre-empting injections whether of horse or sheep erythrocytes were large and it is tempting to suppose that they stress the recipient animal in some way so that fresh immunological responses cannot be registered in lymphoid organs peripheral to the principal reaction site. Although stress may account, at least in part, for the non-specificity of the early pre-emption effect, it is unlikely that the later stages of pre-emption can be explained in this way.

Passively administered antibody has been shown both to enhance and suppress immunity¹⁵⁻¹⁸ although the former property seems to be non-specific and dependent mainly on macroglobulin molecules¹⁹. It might therefore be supposed that, whatever the mechanism for pre-emption during the first 4 days, thereafter circulating 7S antibody specifically inhibits the capacity of cells in peripheral lymph nodes to respond to the pre-empting antigen, but does not affect the response to supposedly non cross-reacting antigens. Without further experimentation this argument must suffice to introduce the phenomenon of pre-emption. Its general relationship to antigenic competition is clear as are its corollaries for the animal which is in the throes of a massive immune response to a systematized antigen. It is also tempting to postulate that a similar mechanism may be operating in the prolongation of homograft survival effected by pretreatment of the recipients with donor blood²⁰, donor bone marrow^{21,22}, or a combination of donor spleen cells and passive antibody specifically directed against the graft²³.

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Two Types of Phototactic Behaviour in Anuran Amphibians

EARLY studies, such as those of Pearse¹ just after the turn of the century, suggested that anuran amphibians face towards and move to light, particularly light from the portion of the visible spectrum that the human observer sees as blue. Muntz has carried out both behavioural² and physiological³ experiments that suggest this phototactic behaviour in species of the genus *Rana* is a true colour preference, rather than a response based on apparent brightness. Chapman's⁴ experiments confirm this conclusion for *Rana catesbeiana*. Because the functional significance of this "blue preference" is still a matter of speculation, we undertook to study anurans outside the family Ranidae to see if other sorts of preferences exist in anurans. Although this comparative study has only begun, we have discovered a different sort of phototactic behaviour, which we here compare with our own replications of the previous work on *Rana*.

The methods of testing spectral and brightness preferences, including the design of critical tests to establish a true colour preference, follow the criteria suggested by Hailman, and the experimental apparatus and procedure are similar to those used by Muntz². A frog is placed in a dark apparatus facing two transilluminated screens and allowed to jump spontaneously towards one (or is touched lightly near the cloaca with a blunted point in order to stimulate a jump). When the animal crosses an arbitrary criterion line, the response is scored as a choice. The criterion lines are established such that when the frog crosses one, it can see only the screen toward which it has been moving, the other screen being obscured by an opaque centre dividing panel. In a given series of spectral or intensity tests, each stimulus is paired with each other stimulus in both left-right and right-left configurations. For six distinct stimuli there are thus thirty different combinations presented in randomized sequence to each frog. Each individual anuran used is presented with a different randomized sequence for its thirty trials.

In the spectral experiments we used six relatively monochromatic stimuli created by filtering light from a 500 W tungsten filament projection lamp through Kodak Wratten glass-encased gelatine filters (numbers 70, 72B, 73, 74, 75 and 76), the dominant wavelengths of which are distributed across the visible spectrum⁵. In the intensity experiments the stimuli consisted of six intensities, ranging from unfiltered light—from the projector (intensity 0) to "dark apparatus" conditions with a shutter imposed in the light beam (4.4 log units below unfiltered light as measured by a calibrated S & M photoconductive photometer). Intensities at -0.5, -1.0, -2.0 and -3.0 were created by interposing calibrated Wratten neutral density filters in the light beam. The intensity stimuli all therefore have the same white spectrum, whereas the spectral series was not matched closely for physical intensity or apparent brightness (J. P. H., unpublished). In the third series of experiments, designed as crucial tests of whether the spectral response involves a true colour-preference or not, the animals were tested on paired colours of known rank-order intensity. The tungsten source used emitted very little ultraviolet energy, and all of the filters have negligible ultraviolet transmission, so the results cannot be confounded by any sensitivity of the animals to ultraviolet.

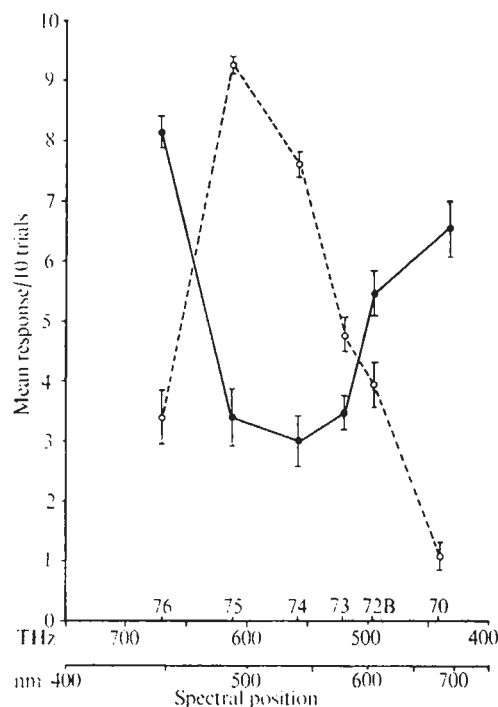


Fig. 1 Results of spectral experiments: mean responses (circles) and one standard error (vertical lines) as a function of spectral position. $N = 15$ individuals for each species. The curve for *Eleutherodactylus rhodopsis* (●—●) generally resembles an inverted spectral sensitivity curve, whereas the curve for *Rana p. pipiens* (○---○) appears to be narrower than any known spectral sensitivity curve. (Spectral position of the stimuli are shown above the abscissa by manufacturer's index number, plotted by dominant wavelength under high-wattage tungsten illumination on a scale of equal frequencies in TeraHerz, with the corresponding scale of wavelengths in nanometers given below).

The entire optical system, including calibrations, and the detailed procedures for keeping and handling of animals will be set forth later when the comparative study is concluded. It is not necessary for purposes of distinguishing the two types of response described here to document the spectral quantum distributions of the stimuli; the chromatic stimuli are therefore shown only by their order in the spectrum, according to the dominant wavelengths⁵ of transmission of the filters. (Roughly, filter 70 appears red to the human eye, 72B orange, 73 greenish-yellow, 74 green, 75 greenish-blue and 76 violet.)

The results of the spectral experiments on the leopard frog *Rana pipiens pipiens* (suborder Diplasiocoela: family Ranidae) and a tropical frog *Eleutherodactylus rhodopsis* (suborder Procoela: family Leptodactylidae) are shown in Fig. 1. The ranid shows the typical preference for short wavelengths (high frequencies) of light, as reported by previous workers¹⁻⁴. The behaviour of the leptodactylid frog, however, describes a distinctly different curve with peaks of response in the extreme ends of the spectrum (violet and red), a curve that generally resembles an inverted curve of spectral sensitivity.

The results of the intensity experiments (Fig. 2) also show striking differences between the species' responses. The ranid is monotonically photopositive, while the leptodactylid is monotonically photonegative over the entire range of intensities possible in our apparatus. On the basis of previous experiments by others we suspected that the ranid spectral response was a true colour preference, whereas the shape of the spectral curve for the leptodactylid frog suggested that it was responding merely to the apparent brightnesses of the stimuli. Critical experiments were devised as follows.

Filter-combinations were chosen for the ranid such that a preferred stimulus from the spectral trials was opposed with a

Table 1 Summary Results of Crucial Colour-versus-Intensity Tests

Colour	Filters used *	Intensity	Mean responses/15 trials colour intensity $P^\dagger$			Preference/10 individuals colour intensity $P^\ddagger$			Significant individual preferences§
<i>Rana pipiens</i> (n = 10)									
75		44+0.4 ND	9.9	5.1	< 0.005	10	0	< 0.001	3
72B		25+0.9 ND	11.8	3.2	< 0.005	10	0	< 0.001	10
<i>Eleutherodactylus rhodopis</i> (n = 10)									
72B		25+0.9 ND	4.2	10.8	< 0.005	0	10	< 0.001	5
73		22+0.9 ND	3.8	11.2	< 0.005	0	10	< 0.001	7

* Kodak Wratten filters by manufacturer's code number plus any added neutral density filters.

† Wilcoxon matched-pairs, signed-ranks test, one-tailed.

‡ Binomial test, one-tailed.

§ Number of individuals whose individual preference was statistically significant at $\alpha = 0.059$.

stimulus having approximately the same spectral dominance as some non-preferred stimulus from the spectral trials. But this second filter was also chosen so that at every frequency (wavelength) it transmitted more light than the first stimulus. So regardless of (a) the ranid's spectral sensitivity, (b) the spectral emission of the light source, and (c) the spectral absorption of the projection screen, the second stimulus *must* appear brighter than (or at least as bright as) the first. Thus, if the animal chooses the dimmer (first) stimulus, it must be using spectral information, and not merely be responding to the apparent brightness of the stimulus. Because the intensity experiment showed that the ranid frog was monotonically photopositive over the entire range of intensities available in the apparatus (from "dark apparatus" to unfiltered white light) the coloured stimuli had to fall in brightness somewhere between those extremes. Filter 75 (which appears bluish to the human eye) was opposed to 44 (blue-green), and 72B (orange) to 25 (red).

Similar critical experiments were devised for the photonegative leptodactylid frog, except that a stimulus preferred in the spectral series was made brighter than a non-preferred colour, by reasoning similar to that used in the design of the

experiments for the ranid species. Filter 22 (orange) was opposed to 73 (greenish-yellow) and 25 (red) to 72B (orange).

The results again show a distinct difference between the species (Table 1). Whereas *Rana p. pipiens* responds in a statistically reliable manner to the dimmer of the two chromatic stimuli in both critical experiments (despite its monotonically photopositive response to white light), *Eleutherodactylus rhodopis* does not choose the brighter of its two chromatic stimuli, but instead responds reliably according to its intensity preference in both experiments. We conclude that *Rana p. pipiens* has a true colour preference in phototaxis, and one so strong that it overcomes the intensity preference, whereas no phototactic colour preference can be demonstrated for *Eleutherodactylus rhodopis* (and if one does exist, it is strongly masked in both critical experiments by the photonegative intensity preference).

There are many ecological and evolutionary differences between these two species, and no functional significance for the two types of responses can be stated at this time. Preliminary results with species in other families suggest that these two responses are the principal types of phototactic behaviour in anurans: (1) photopositive to white light with blue colour-preference, and (2) photonegative to white light with no discernible colour-preference (that is, responding in spectral trials according to the spectral sensitivity of the eye). Both the functional and physiological significance of these two classes of phototactic behaviour may become clearer when we have had an opportunity to test more species.

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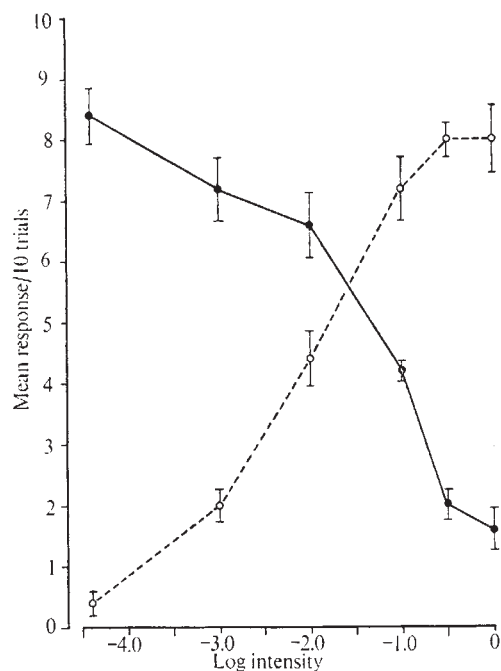


Fig. 2 Results of intensity experiments: mean responses (circles) and one standard error (vertical lines) as a function of the intensity of white light (in log units below unfiltered tungsten source illumination). $N=5$ individuals for each species. *Eleutherodactylus rhodopis* (●—●) is monotonically photonegative, whereas *Rana p. pipiens* (○---○) is monotonically photopositive.

BOOK REVIEWS

Classless Education

Resources for Learning. By L. C. Taylor. (Penguin Education Special.) Pp. 277. (Penguin: Harmondsworth, Middlesex, January 1971.) £2.00.

A FASCINATING browse through possible recipes for learning forms the best parts of this book, another valuable contribution from the Penguin "Education Special" series. Past attempts in Britain, current ones in America, Russia and Sweden—Taylor has a sure touch for the concrete details that keep the reader on firm ground among the treacherous terminologies of comparative education, and he writes with a *brio* that makes for compulsive reading. But the serious purpose is never far below the surface. Taylor is urging us to explore seriously alternatives to teacher-based class teaching which might allow more scope for independent learning.

The need arises from a variety of pressures, chief among them those that result from comprehensive reorganization. (He calls it "comprehension"—but if so, why not "progression" for "progressivism"?) Out of the chaotic profusion of patterns elicited by Circular 10/65, all-through eleven-to-eighteen comprehensives have been the commonest single choice, regardless of the apparent impossibility of supplying all their sixth forms with graduate specialist teachers. In the lower forms, unstreaming is no longer outrageously avant-garde. The time-honoured chalk-and-talk technology can hardly be expected to withstand the resulting conflict between liberty and equality. Somehow we will have to find ways of breaking the lockstep in which a teacher holds a group of thirty or so pupils when he teaches them as a class.

So Taylor looks abroad to see if they have managed to order these things better elsewhere. In America, team teaching—with time divided among large group instruction, small group seminars and individual study—has made some impact. At junior college level, there are attempts to add variety

to programmed learning by equipping learning laboratories. In Russian schools, Taylor found not experimental methods, but the old ones being made to work. When the short school day in the unstreamed comprehensive ends, however, the Pioneer Palace takes over; there volunteer learners stream themselves by choice and ability, and docility is replaced by zest.

Sweden, with its recent experience of going comprehensive, offers a closer parallel. Voluntary setting having proved unworkable and given way to a commitment to unstreaming, the need for individualized work has become urgent. The natural basis for developing it was found in the correspondence courses, already of high repute. Nothing elsewhere, according to Taylor, is as sophisticated and well tested as the IMU maths course for 13 to 16 year olds.

One waits impatiently to see what positive prescriptions for here and now will emerge from so sane an appreciation of the ideals and foibles of others. The end of the book turns out, however, to be something of a disappointment; the sparkle of the writing becomes dimmed by that tinge of pious aspiration which educational innovators cannot avoid when they come to talk about their own innovations rather than other people's. Of course one accepts that this is in no sense a full report of the Nuffield Resources for Learning Project, of which the author is a co-director. But the bare list of the names of the members of the Consultative Committee and the staff feels like a snub to the expectant reader.

Taylor suggests that the secondary sector lags behind both primary and post-school education in independent learning. In some ways he is over-optimistic. Science in higher and further education suffers, by comparison with non-scientific subjects, from the relative lack of independent work. As university teachers face pressures to re-structure their teaching efforts, they too could find useful reading here.

F. R. JEVONS

Advent of Domestication

Geography of Domestication. By Erich Isaac. (Foundations of Cultural Geography Series.) Pp. viii + 132. (Prentice-Hall: Englewood Cliffs, NJ, December 1970.) \$6.50 boards; \$2.95 paper.

WITH the benefit of hindsight we can see the domestication of plants and animals as the most significant achievement in the history of our species. It has enabled mankind to develop some independence of his environment unparalleled in any other species, and allowed that other unique human attribute, intellect, to operate in a manner which would not have been possible without such independence. Yet, in spite of much attention to the question, the processes of domestication and the factors which led to them are still not understood.

Domestication does not fall tidily into any one discipline, but ranges over, *inter alia*, anthropology, archaeology, biology, ethology, history and religion. Consequently, research has produced a wealth of evidence coming from many difference angles, often uncoordinated across disciplinary boundaries. There therefore arises a dual need in publication: the restricted technical paper and the coordinating review. A modern development, brought about by the very extensiveness of new work, is the symposium volume, and in this field there has recently been at least one major publication of this type. Such a volume, however, inevitably lacks the coordination and synthesis possible in a review such as the one by Professor Erich Isaac entitled *Geography of Domestication*.

The book is short enough to read quickly and coherently (though the wealth of footnote references makes detailed study possible), and when this is done, the problems, contradictions and hypotheses of greater or less substance are seen in valuable perspective. The evidence of archaeology, man's implements, plants and animals is examined in the book's six chapters, and arguments developed about the

supposed place or places of the original domestications. While surveying the claims of other areas to be the cradle of domestication, the author soon decides that the domestication of most animals and plants, perhaps even of root crops, was centred on the Near East. While allowing that the evidence of independent domestication in the New World is at present strong, he does not bar the possibility that even here Old World influences could have played a part. Failing to find any other cause which satisfactorily explains such a drastic change in human behaviour, Professor Isaac agrees with Eduard Hahn that domestication, particularly of cattle, may have had a religious motive. This theory would of course be considerably weakened if it were ever proved that domestication happened at different times and in different places, but the author sees no reliable evidence for this. Indeed, to him the evidence suggests the opposite.

Nevertheless, reading this book as a whole, one wonders whether some of the recent work has been adequately considered. In particular, the ecological background to domestication is scarcely mentioned, and the growing contribution of pollen analysis to our knowledge of late Pleistocene conditions in the critical areas is ignored. In a field where facts are outnumbered by hypotheses, and where new facts are constantly challenging our assumptions (some of Professor Isaac's included), no review can afford to ignore such sources. Such limitations notwithstanding, this work must be regarded as a valuable and welcome contribution to an important subject and one whose price is happily within reach of the average student. G. W. DIMBLEBY

Sea Structure

Circumpolar Characteristics of Antarctic Waters. (Antarctic Map Folio Series. Folio 13.) Sound Channels in Antarctic Waters. Edited by A. L. Gordon, R. D. Goldberg and K. Hunkins. Pp. 6+19 plates. (American Geographical Society: New York, September 1970. Published under a contract with the US National Science Foundation.) \$12.00.

THE American Geographical Society's Antarctic Map Folio Series, made possible by financial support from the Office of Antarctic Programs of the National Science Foundation, summarizes present knowledge of the Antarctic. Folios issued so far deal with surveys, meteorology, climatology, geology and glaciology, and this is the fourth to deal with the sea. It is the second to deal with water structure and

movements, Folio 6 having dealt with the Falkland and Pacific sectors between 20°W and 170°W, where the US research vessel *Eltanin* has done most of her work. The latest maps cover the whole of the ocean south of latitude 40°S. They present the data in three ways: first, contoured maps of temperature, salinity and oxygen content at depths of 20, 200, 500, 1,000, 2,000 and 3,000 m; second, average values of the same variables for 'squares' bounded by 5° latitude and 10° longitude, and for additional depths of 4,000 and 5,000 metres; and third, seven contoured meridional, vertical profiles, spaced round the circumpolar ocean but with rather a gap in the west Pacific sector. The numbers on the map which show the locations of the profiles do not correspond to those on the drawings: they should be phased round one profile to the west. There are also diagrams showing the depth distribution of the main sound channel, where underwater sounds might be heard at great distances because multiple refractions concentrate the sound near the depth of minimum sound velocity.

When interpreting the maps of temperature, salinity and oxygen content one has to remember that the water movements that do much to control the distribution of temperature and salinity at all depths are not themselves horizontal. The temperature and salinity maps for 200 metres must, for example, show a circumpolar belt of low temperature, low salinity and high oxygen content, north of the Antarctic convergence where the Antarctic surface water sinks through the 200 metre level to a greater depth, and there must be higher temperatures, higher salinities and lower oxygen content some 10 degrees farther south where deep water climbs above 200 metres. In this respect the earlier Folio 6 is more easily interpreted because it maps each variable in the layers of temperature, salinity and oxygen minima and maxima instead of at fixed depths. But it is useful to have the new comprehensive display and the vertical profiles are a great help. It was done chiefly by computer methods, and it will be particularly useful for studies of meridional and vertical exchanges. The averaged data also add to the overall picture at the 4,000 and 5,000 metre depths where observations are still rather scarce. G. E. R. DEACON

Story of a Moor

Dartmoor: a New Study. Edited by Crispin Gill. Pp. 314+47 plates. (David and Charles: Newton Abbot, November 1970.) £3.75.

CRISPIN GILL has edited a symposium of studies on aspects of Dartmoor intended to fill the need for a modern definitive

study. The book is largely concerned with the industrial development of the region with introductory chapters on the physical environment and prehistory. The burning political issues of the contemporary use of Dartmoor were unfortunately outside the scope of this survey, although in the words of its editor "the danger of each proposed development should be costed not just in its immediate amenity effect but in its cost to the succeeding generations as yet unborn".

Since the earliest days of Man's habitation of Dartmoor, its natural resources have been exploited. The extractive industries predominated, particularly those of tin, lead, copper and iron, while the use of granite for building and peat for fuel attracted both ambitious entrepreneurs and local craftsmen. Farming, however, has always been and still remains the most important aspect of the Moor's economy and has largely been unperturbed by the socio-economic changes to which other industries failed to adapt. Modern industrial Britain requires from Dartmoor only its china clay, which is exported in large quantities, because metal mining and the cloth and corn mills have largely been abandoned.

Dartmoor, nevertheless, has more to offer, for its natural beauty is still preserved by National Parks legislation. The coming of the railway to Exeter in 1844 with its branch lines pushing into the remote villages, together with the advent of coach services later in the century, opened the Moor to visitors; later, as the advent of the private motor car brought easy travel within the reach of a large part of the population, the Moor became freely accessible to many in a way which had been hitherto impossible. Opportunities were therefore readily available not simply for relaxation but also for serious study. From the descriptions in this book, the reader is made aware of the vast amount of archaeological work that has been undertaken in the area: extensive excavations began in 1893, making discoveries which form the basis of our knowledge of Bronze Age and Early Iron Age cultures. The present archaeological emphasis, however, is upon a more systematic study of prehistoric settlements, and an up to date analysis is given of material hitherto only published in specialized archaeological journals.

This study, nevertheless, is not intended simply for the specialist, although he will find it well documented and scholarly in its presentation. It contains much to interest the general reader, particularly one acquainted with the region. This is indeed a fine book and a worthy successor to earlier, and by now classic, studies of Dartmoor.

DIANNE E. WOODHAM

Genes of Change

Biochemistry of Differentiation. By Charles A. Pasternak. Pp. xiii+189. (Wiley-Interscience: London and New York, 1970.) £2.75.

THE aim of this book is to explain in fairly simple terms the biochemical approaches to understanding differentiation. The level can be fairly described as second year undergraduate, although Dr Pasternak says that the book is based on a lecture course given to third year students at Oxford. Although the author seems to be aware of the difficulties of developing a coherent theme on such a diffuse subject, he has perhaps remained too true to his lectures and has not been entirely successful in translating them into written form.

Most of the book seems to be taken up with establishing what most workers would now regard as an *ex hypothesi* starting point; that differentiation is achieved by the controlled action of specific genes, although this control may be exerted either of transcription or of translation (or indeed, in between—for example, at the level of the nuclear membrane). The discussion of differentiation in sea urchins and frogs is a good example of this concentration on symptoms rather than causes. Essentially, this chapter describes what is known about nucleic acid and protein synthesis during successive stages of embryogenesis. But while this is all doubtless very important, the real question remains unanswered, and indeed, unasked: what controls this differential gene action?

After six chapters leading to the conclusion that differentiation involves the specific syntheses of certain enzymes and structural proteins, Pasternak comes to two chapters concerned with the control of these processes. The first is devoted to excluding the idea that genes are differentially lost from differentiated cells.

It is only the final chapter which attacks the nub of the problem; but this general discussion of the levels at which control may be exercised both misses the central issue and is too simplified and compressed in what it does say. Can differentiation and development be explained solely by actions encoded in the genetic information of DNA or are there independent forms of information which cannot be reduced to nucleotide sequences in DNA, perhaps gradients in cells or positional information in membrane structure? Pasternak is too readily concerned with the mechanisms of differential gene action and not enough with the wider issue of whether these mechanisms, although necessary, are sufficient. This issue may be as much philosophical as scientific, and certainly one does not expect a full ex-

position; but the problem should at least be pointed out.

And the discussion about control of gene action leaves much to be desired. The section about whether histones are gene repressors, for example, although accurate is rather sketchy. Nucleic acid hybridization experiments are mentioned, but there is no caveat to explain why they are of only limited reliability in higher organisms. The problem of accounting for excess DNA—surely a central one—is not mentioned; the puffing of salivary gland chromosomes and its control by ecdysone and ionic levels is mentioned only in passing.

Surely a book on this topic should mention the Britten-Davidson model, if only to point out the very considerable drawbacks which there are to explaining the control of gene action in higher organisms by models analogous to those for bacterial systems? It is notable that 'operon' is not even cited in the index; it may perhaps be misguided to look for operons in eukaryotes, but so many workers think along these lines that it is imperative to explain how and why bacterial-like control systems relate or fail to relate to the systems which Pasternak discusses.

But of course, it is difficult to present an all round view of differentiation, especially in so small a book as this. So notwithstanding its defects, the book has its uses, to define some of the sorts of approach used to tackle differentiation. Certainly, it will confer a familiarity with the terms used and the systems which are popular. To end on a more mundane note, however, the volume seems expensive for so superficial a view of the subject, and it is a pity that it was not issued in a much cheaper edition.

BENJAMIN LEWIN

Physiology of Vision

Physiology of the Retina and Visual Pathway. By G. S. Brindley. Second edition. (Monographs of the Physiological Society.) Pp. xi+315. (Arnold: London, November 1970.) £4.50.

THIS second edition of Professor Brindley's well-known book, first published in 1960, has been carefully revised and the additional literature up to the time of going to press (September 1969) is well covered.

The opening chapter on the photochemistry of the retina contains an account of the changes that occur following irradiation of visual pigment. It deals satisfactorily with the complexities of the subject, and sufficient references are given to introduce the reader to the points of conflict in the literature. Greater prominence might have been given, however, to the views

of Bridges on the thermal regeneration of visual pigment from low-temperature photoproducts. Brindley also states that Lythgoe and Quilliam's transient orange is almost certainly pararhodopsin (metarhodopsin₄₆₅ or metarhodopsin III), though it is arguable that the transient orange stage in the bleaching of frog rhodopsin includes metarhodopsin I. The three chapters that follow—on the signals of rods and cones, the nerve cells of the retina, and the central pathway of vision—are wide ranging and a valuable guide to a diverse literature that is scattered both in time and language. These accounts I found to be both satisfying and stimulating.

The second half of the book is concerned with sensory experiments. After a brief introduction to the important distinction between Class A and Class B observations (those that respectively can and cannot be expressed as the identity or non-identity of two sensations) there is a chapter on visual illusions, that is, sensory observations that involve neither matching nor threshold. This clears the way for an excellent discussion of spatial, temporal and adaptational aspects of the visual threshold.

The concluding chapter is on human colour vision, with a detailed statement of trichromacy and its implications. Poor Hering gets short shrift. . . . "The arguments for Hering's theory may seem to some readers so feeble that they wonder why I mention the theory". Brindley rightly makes the point that the principal plank in Hering's theory is his argument from the appearance of colours—a Class B phenomenon which "though non-rigorous is not necessarily bad". Brindley's view is "that we must postpone judgement until it has been determined whether those aspects of the subjective appearance of colours from which Hering argued are independent of the environment of the subjects during childhood. Among terrestrial natural objects, green and blue are commoner colours than blue-green, and many languages have simple words for green and blue and a compound word for blue-green. It might be that for a child brought up in an environment where all these three colours were equally common, and with a mother-tongue in which all three had equally simple names, the assertions from which Hering argued would not hold. . . . But if the subjective simplicity of red, yellow, green and blue and the incompatibility of redness with greenness and of blueness with yellowness prove to be independent of upbringing, then Hering's argument is vindicated." Brindley's argument seems to me to imply a less than credible tyranny of words.

H. J. A. DARTNALL

Chemical Masking

Masking and Demasking of Chemical Reactions: Theoretical Aspects and Practical Applications. By D. D. Perrin. (Chemical Analysis: a Series of Monographs on Analytical Chemistry and its Applications, Vol. 33.) Pp. x+211. (Wiley-Interscience: New York and London, November 1970.) £6.25.

THE masking of a chemical reaction is defined at the outset of this book as the effect whereby "the concentration of a given ion in solution may be so diminished by the addition of substances which unite with the ion to form complex salts, that an ion product sufficient to form a precipitate or cause a colour reaction is no longer obtained". This is Feigl's definition of 1936. The author goes on to comment in his introduction, "In this way without having resort to physical separations, Feigl was able to make spot-tests highly selective or even specific".

Generally speaking 'masking' has come to mean the prevention of the normal reaction of an ion upon addition of a reagent with which it reacts, by the presence of a (deliberately) added reagent with which it forms a soluble complex. Thus the prevention of normal reactions of, say, the barium ion by previous addition of sulphate ion would not normally be regarded as the 'masking' of barium ion because according to Feigl's definition, first, a complexing agent has not been used, and second, separation has occurred, that is a phase separation has occurred even though filtration has not been employed.

Much confusion exists in the literature on this, however, because some authors apparently do not regard precipitation without physical removal of the precipitate as physical separation. Thus some precipitation processes are included in this book as masking actions though clearly the author has largely avoided listing precipitating agents as masking agents. It is, however, a pity that he did not exclude such separation processes completely.

Again, however, Feigl's original definition does not quite hold because pH control is regarded as a masking process and so also is oxidation or reduction. In the latter two instances this appears to be a reasonable extension of Feigl's definition. The essential feature of the masking action is clearly that no physical separation is involved, that everything remains in solution when one is considering solution reactions and the like.

This is the first monograph on masking agents though very many books on spectrophotometry, complexometry and so on do give quite extensive and sometimes even monographic treatment of the subject in relation to their own topics. The author has scarcely

acknowledged the existence of these, though books such as Sandell's *Colorimetric Determination of Traces of Metals*, to mention only one of many, have served two or three generations of students very successfully in this respect.

This book, however, is an excellent one and the author has not only done us all an excellent service in writing it, but has moreover written it extremely well. The presentation is lucid and the material is arranged very successfully according to techniques: gravimetry, titrimetry, absorption spectrophotometry, ion exchange, solvent extraction, electroanalysis, and according to applications, for example, industry and biology. The author's comment that many of the techniques described in the text have been overtaken by the evolution of more physical than chemical techniques such as neutron activation and atomic absorption analysis is not really borne out in practice because both these techniques, particularly the former, rely quite heavily on separation processes such as solvent extraction, or ion exchange in their application to real everyday analytical problems.

The use of masking agents is undoubtedly here to stay for many years to come in practically all branches of analytical science. This book is a first class one. No educational library should be without it nor should any laboratory where the analysis of metals is practised at macro, micro or trace level.

T. S. WEST

Electron Optics

Electron Beams, Lenses and Optics. Vols. 1 and 2. By A. B. El-Kareh and J. C. J. El-Kareh. Vol. 1: Pp. xiii+411. Vol. 2: Pp. xi+320. (Academic: New York and London, September 1970.) Vol. 1: £9.20; Vol. 2: £8.15.

THE first volume of this work is largely concerned with the first-order properties of the electrostatic two-cylinder lens, the einzel lens and the round magnetic lens with symmetrical polepieces. The second volume presents a full derivation of the Scherzer formulae for the third-order aberration coefficients, followed by a detailed account of the spherical and chromatic aberration coefficients for the two-cylinder lens and the symmetrical magnetic lens (after Glaser). The beam-spread curve and the Airy diffraction formula are also derived and discussed briefly.

The work is narrowly conceived and its contents justify neither its title, its length nor its price. Nearly four hundred pages are taken up with tables of computed values of cardinal points and axial aberration coefficients of simple lenses. These are of somewhat

doubtful value for they are derived almost exclusively from well known analytical approximations. The possibility of solving the Laplace and trajectory equations numerically is hardly mentioned, and yet numerical techniques are now almost universally adopted in serious electron optical design.

Apart from the tables of data, the space is mostly taken up with line by line mathematical derivations of the analytical formulae on which the tables are based. This represents a rather extravagant use of space in an expensively produced book. The whole enterprise suffers from a lack of balance and perspective. The correction of spherical aberration, which has long been a chief preoccupation in electron optics, is dismissed in two sentences. "Several methods have been proposed to cancel this defect; however, none have so far materialized. Therefore we shall not dwell on this particular subject." Experimental data figure little, and discussions of techniques or applications are almost entirely absent, as are numerically computed results (though Vine's computations of einzel lenses are included erroneously as experimental data).

The bibliography is practically nonexistent, on the grounds that "readily available sources fill this need". This is a serious omission in a work of any pretensions, but is typical of this disappointing book, in which the contents are so much overshadowed by what has been left out.

M. E. BARNETT

Radical Reactions

Free-Radical Chain Reactions. By Earl S. Huyser. Pp. vii+387. (Wiley-Interscience: New York and London, October 1970.) £9.50.

THIS book is not intended for the expert in free-radical chemistry or indeed the physical chemist whose interest is primarily in kinetics, though the latter will find many interesting examples on which to reflect. For the organic chemist who wishes to know something about the subject of free-radical chemistry this book provides a useful if somewhat idiosyncratic presentation. It is occasionally repetitious and there are some curious omissions. Some ideas are presented with little experimental evidence, and at times some of the distinctions—for example, between abstraction and displacement reactions—are a little confusing. As a whole, however, the book can be recommended to the chemists for whom it is intended, though the very high price suggests they will wish to consult it in their libraries rather than purchase a personal copy.

H. M. FREY

CORRESPONDENCE

Wastage of Skill in the US

SIR,—The doctrine has been announced by the US government, and publicly supported by some politicians, scientific administrators and even scientists, that the present scarcity of jobs for technically trained men and women is the result of training too many people. I argue that this shortage is artificial, unnecessary, and caused by deliberate governmental policy to save money.

We have been through this before in the great depression of the 1930s. The cause is known, also the cure. Essential national needs are neglected on all sides, for example, in staffing the ever-expanding institutions of higher education, improving the nation's transportation system in all its forms, salvaging what is left of the inner cities, trying to prevent the impending crisis in energy supplies, bringing health care to every citizen whether rich or poor or in between, conserving our natural resources, and providing an adequate standard of living for all in the richest nation on Earth.

The answer is to carry on useful programmes, financed by both public and private sources, including both basic and applied research in universities, governmental laboratories and industrial organizations. To do these things we need all the trained people we can get, and at all levels. Ours is a technological culture and the way to keep it going is by expanded scientific effort, not by killing the goose that lays the golden eggs.

There is no fat cat more content to put up with things as they are than an academic with tenure. I am deeply disturbed that many of my colleagues seem to accept without question the doctrine of over-production of trained people, when the true state of affairs is deliberate

under-utilization, reversible by US governmental action. Where would the money come from? The answer is obvious. Stop the war that has killed or wounded a quarter of a million US men, squandered our resources, ruined Vietnam and degraded the US before the world. All this has not cost either China or Russia a single man, nor has it hindered their scientific and technical growth. We in the US, on the other hand, are seeing our strength waste away under misguided governmental policy.

Yours faithfully,

ROBERT E. JOHNSON

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Biochemistry: *IJB* and *EJB*

SIR,—A recent issue of *Nature* (226, 51; 1970) contains a review by Dr J. H. Morris of the newly launched *International Journal of Biochemistry* (*IJB*). The review concludes with the statement that "So long as proliferation is the watchword of communication, the only choice is between more issues of the same journals or the addition of new ones. To judge from the progress of recent starters such as the *European Journal of Biochemistry* (*EJB*), the subject is big enough for all comers and their respective fates will ultimately depend on the standard of excellence they attain".

I was closely involved with the creation of the *EJB*, being the chairman of the committee that recommended its formation, and secretary-general of the Federation of European Biochemical Societies at the time that this recommendation was

accepted. I do not think that there should be any comparison of the *EJB* with *IJB*.

The *EJB* is not a "new" journal, in the sense that its creation did not add to the number of existing journals. On the proposal of the Gesellschaft für Biologische Chemie, Springer-Verlag agreed with FEBS to abandon publication of the *Biochemische Zeitschrift*, with its long and distinguished history, in return for becoming publishers of the *EJB*.

There are, in addition, a number of equally important criteria that serve to distinguish the *EJB* from the *IJB*. The *EJB* was only established after the widest consultation within the member societies of FEBS, representing several thousand European biochemists. Its scientific policy is entirely in the control of the federated societies and the continuity of that control is assured through the FEBS Publications Committee, and the journal's editorial and advisory boards—numbering some 70 biochemists from Europe and the United States. The situation of the *IJB* is very different. It is a private venture with an editorial board of two, from the same university department. It received at least some of its start in life from manuscripts originally submitted to *Comparative Biochemistry and Physiology* and resubmitted to the *IJB*. (The two journals share a common editor.) It is relevant here to note that in his review of the *IJB* Dr Morris pointed out that "Each biochemical journal seems to acquire for itself a characteristic slant—the accent in this first issue is very much on comparative enzymology and metabolism".

Yours faithfully,

W. J. WHELAN

*University of Miami,
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Florida 33152*

Announcements

University News

Mark Ptashne has been appointed professor of biochemistry and molecular biology at Harvard University.

Mrs Kathleen Bell has been appointed to the second chair of social studies in the University of Newcastle upon Tyne.

Dr L. J. Challis has been appointed to the third chair of physics and Mr E. F. Williams has been appointed special professor of food hygiene in the Department of Applied Biochemistry and Nutrition, in the University of Nottingham.

Appointments

Professor P. G. Stein, Queen's College, Cambridge, has been appointed to the University Grants Committee.

Professor A. R. J. P. Ubbelohde, Imperial College, London, has been reappointed a member of the Agricultural Research Council.

Mr C. L. Old, group education officer to Vickers Limited, has been appointed assistant secretary to the Council of Engineering Institutions, in succession to Rear-Admiral I. G. Aylen.

Miscellaneous

A PRIZE of 5,000 Swiss francs will be awarded by the International Health Foundation for the best study submitted

to the foundation on the subject "Gonadal Function in Man between 45 and 55 Years of Age". Papers may deal with any aspect of this subject—clinical, social, or biochemical—and should contain new and original ideas. They should be written in English and should not exceed 2,500 words in length. Quality rather than quantity will influence the final selection. Papers, together with a short curriculum vitae of the author, should be sent to the International Health Foundation, 14 rue de Hollande, 1204 Geneva, Switzerland, to arrive by June 1, 1971. The results will be announced in August 1971. An additional prize of 2,000 Swiss francs may be awarded for the best paper submitted by an author under 35 if the quality of the entries permits.

British Diary

Monday, March 22

Air Pollution (6 p.m.) Dr A. J. Robinson, Royal Society of Arts, at John Adam Street, London WC2. (Second of four Cantor Lectures on "Pollution".)

Artificial and Natural Pollution Tests on Outdoor 400 kV Substation Insulators (5.30 p.m.) Mr C. H. A. Ely, Mr R. G. Kingston and Mr P. J. Lambeth. Institution of Electrical Engineers, at Savoy Place, London WC2.

Circuit Theory (vacation school, twelve days) Institution of Electrical Engineers, at the University College of North Wales, Bangor, North Wales.

Ferrite Microstrips (10.30 a.m. colloquium) Institution of Electrical Engineers, at Savoy Place, London WC2.

Tuesday, March 23

Direct View Storage Tube Displays (6.30 p.m.) Mr A. B. E. Ellis, Institution of Electronic and Radio Engineers, at Colchester.

Negative Ions (four-day conference) Institute of Physics and the Physical Society, at the University of Liverpool.

Nineteenth Century Founders of the Zoological Collections (6.30 p.m.) Mr A. E. Gunther, British Museum (Natural History) and the Victorian Society, in the Lecture Hall, British Museum (Natural History), Cromwell Road, London SW7.

Radiological Protection and Society (10 a.m.) Society for Radiological Protection, at London.

The Nature of Automation as Part of the Evolutionary Process (5.30 p.m. discussion) Institution of Electrical Engineers, at Savoy Place, London WC2.

The Pattern of Energy Consumption in the 1970s (2.30 p.m. colloquium) Institution of Electrical Engineers, at Savoy Place, London WC2.

Wednesday, March 24

Automation Engineering (6.30 p.m.) Professor P. K. M. M'Pherson, Institution of Electrical Engineers, London Gradu-

ate and Student Section, at Savoy Place, London WC2.

Combustion Hazards in Industry (7.30 p.m.) Mr Murray, Oil and Colour Chemists' Association, at the Carlton Hotel, North Bridge, Edinburgh.

Engineer to Entrepreneur (6 p.m. discussion) Institution of Electronic and Radio Engineers, at 9 Bedford Square, London WC1.

Factors Influencing the Choice of a Mill for a Specific Purpose (5.30 p.m.) Professor H. E. Rose, University of London, at King's College, Strand, London WC2. (Last of four lectures on "An Introduction to Comminution Technology".)

Structure and Growth of Thin Films (5th Thin Films Conference), Institute of Physics and the Physical Society, at the University, Southampton.

The Biochemistry of the Covalent Cross-linking in Collagen (5.30 p.m.) Dr Gerald Mechanic, University of London, at the Royal Dental Hospital of London School of Dental Surgery, Leicester Square, London WC2.

The Influence of Technology on the Social Structure (6 p.m.) The Right Rev. I. T. Ramsey, Royal Society of Arts, at John Adam Street, London WC2. (Trueman Wood Lecture.)

Whither Nuclear Power? (5.30 p.m.) Mr E. S. Booth, FRS, Institution of Electrical Engineers, at Savoy Place, London WC2.

Thursday, March 25

Integrated Circuits in Hi-Fi Systems (7.30 p.m.) Mr B. A. Reed, Institution of Electronic and Radio Engineers, at the J. J. Thomson Laboratory, University of Reading, Whiteknights Park, Reading, Berks.

Problems in the Development and Manufacture of Perfumes Toiletries (7.30 p.m.) Mr K. J. Aynsley, British Society of Perfumers, jointly with the Society of Cosmetic Chemists GB, at the Institution of Electrical Engineers, Savoy Place, London WC2.

Techniques for Separating Biological Signals from Biological Noise (2.30 p.m. discussion) Institution of Electrical Engineers, at University College London, Gower Street, London WC1.

Friday, March 26

Aerosols (7.15 p.m.) Mr D. T. Trist, Oil and Colour Chemists' Association, at the Royal Hotel, Bristol.

Glow Discharge—Electron Beams (5.30 p.m. discussion) Institution of Electrical Engineers, at Savoy Place, London WC2.

Starch: Granule Structure and Technology (symposium) Society of Chemical Industry, Food Group, at the School of Pharmacy, Brunswick Square, London WC1.

Trends in the Design and Application of High Voltage HRC Fuses (5.30 p.m.) Mr J. Feenan, Institution of Electrical Engineers, at Savoy Place, London WC2.

Monday, March 29

Land Pollution (6 p.m.) Sir Frank Fraser Darling, Royal Society of Arts, at John Adam Street, Adelphi, London WC2. (Third of four Cantor Lectures on "Problems of Pollution".)

Reports and Publications

not included in the monthly Books Supplement

Other Countries

Museum of Comparative Zoology, Harvard University. Breviora. No. 354: Taxonomic and Ecological Notes on some Middle and South American Lizards of the Genus *Ameiva* (Teiidae). By Arthur C. Echternacht. Pp. 9. No. 355: Generic Relations and Speciation Patterns in the Caracaras (Aves: Falconidae). By F. Vuilleumier. Pp. 29. No. 356: On New Species in a New Earthworm Genus from Puerto Rico. By G. E. Gates. Pp. 11. No. 357: A Review of the Fossil Pelomedusidae (Testudines, Pleurodira) of Asia. By Roger C. Wood. Pp. 24. No. 358: South American Anoles: *Anolis apollinaris* Boulenger 1919, a Relative of *A. biporcatus* Wiegmann (Sauria, Iguanidae). By Ernest E. Williams. Pp. 11. No. 359: The Swimbladder as a Juvenile Organ in Stromateoid Fishes. By Michael H. Horn. Pp. 9. No. 360: Mammals from the Early Cenozoic of Chubut, Argentina. By George Gaylord Simpson. Pp. 13. No. 361: Additions to Knowledge of the Argrolagidae (Mammalia, Marsupialia) from the Late Cenozoic of Argentina. By George Gaylord Simpson. Pp. 10. No. 362: Addition to Knowledge of *Groeberia* (Mammalia, Marsupialia) from the Mid-Cenozoic of Argentina. By George Gaylord Simpson. Pp. 17. (Cambridge, Mass.: Museum of Comparative Zoology, Harvard University, 1970.) [11]

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